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CONTENTS.

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*LIFE AND CHEMISTRY.**

As I look into the eyes of those before me, I can not but have the feelings of Moleschott in his address at the reopening of the University of Rome when he found himself 'in the face of an audience whom he had nothing to teach, but from whom he had much to learn.'

An imposing knowledge of the distinctions attained by my two distinguished predecessors, perhaps should be depressing; for it is no longer an investment here to forge ahead, but an investment to keep up. On the contrary, their unseen presences stand not as spectres, but as gracious good guardians.

It becomes necessary in the outset to confess to an inner consciousness, that we know. *How* we need not consider for the present further than that the internal thought-centers, association or sense-centers take impressions from the external world and transform them into presentations, which automatically, as it were, frame themselves into concepts. The 'ultimate nature of reality' is not of immediate moment.

Doubtless, man from his earliest experience has speculated on the origin and perpetuation of life, that is, nature. This period is no exception. President Jordan has written, 'whatever else may be said of it, this is certainly the age of deliberate scrutiny of origins and destiny.'

Kant, among many things, wrote 'Only

* Inaugural lecture on the assumption of the head professorship of chemistry in the College of the City of New York, February 28, 1905.

in experience is there truth.' And Haeckel, in his persistent advocacy of the great monistic system, based upon the unity of nature and the unity of science, has asserted, 'All natural science is philosophy, and all true philosophy is natural science. All true science is natural philosophy.'

The great pile at a distance presents itself to the mental perspective in barest outline. As the imaginer draws nigh, the multitudinous variety of the structure is limned on the intellectual horizon. The enormity of the edifice is overmastering. "The larger grows the sphere of knowledge, the greater becomes its area of contact with the unknown." Various crafts must needs arise to disentangle its intricacies.

Numerous subdivisions of natural science have resulted. Each gives more and more to its specialty. At times, in the close scrutiny, the investigator is so near that he fails to grasp the whole and see their relationship. It is unfortunately a fact, as Pearson says, that 'no man whose nose is always on the details of observation is a safe fact-gatherer, while no one whose head is too high above such necessary drudgery is a safe generalizer.'

The complexity of nature seems boundless. "Boundless inward in the atom; boundless outward in the whole."

Biology is the science of life. Elsewhere I have asserted that biology is the application of physics and chemistry to living matter. It requires little knowledge to show that these divisions of science are purely arbitrary. After all, they are not so much divisions or parts of science, as they are simply different methods of looking at the same mystery. While Laplace in his mechanical conception of the world asserted that the progress of nature could be foretold for all eternity if the masses, their position and initial velocities were given, Mach has well said, "Physical science does not pretend to be a complete view of the world; it simply claims that it is working towards such a complete view in the future. The highest philosophy of the scientific investigator is precisely this toleration of an incomplete conception of the world and the preference for it rather than for an apparently perfect but inadequate conception." It is not, therefore, gross temerity which prompts the chemist to give thought to the relations of life and chemistry, for life is chemistry, chemistry applied. It may border on rashness, however, to give utterance to those thoughts, as masters have lost themselves in such contemplations.

Chemistry has taught man to know aright the requirements of mother earth, the conditions which must be fulfilled to ensure bountiful crops. No longer need virgins be sacrificed to the genius of maize, as was done by some tribes of American Indians, in order to plead for a generous yield of the life-sustaining cereal. (Wiechmann.)

While science appeals to us from a most practical point of view and especially is that true for chemistry, it is not to that phase of the subject that I desire to direct your attention.

Before Darwin there was more anticipation, but some interpretation, of nature. Although preceded by Heraclitus and Empedocles, Aristotle appears to have laid the foundations of biology. For fifteen centuries, with limited exceptions, they were unbuilt upon. Then there began the gradual growth of the scientific renascence, culminating in the Encyclopædists. Science, however, does 'not consist solely in the description of observed facts.'

Biological history for our purposes may be schematized as follows: organism, organ, tissue, cell protoplasm. Altmann's visible granules, Flemming's threads, Frommann's skeleton, Bütschli's honeycomb, according to Haeckel, are but sec-

ondary products of the differentiation of the plasma.

Latterly, Osler has said, "Around the nature of cell-organization the battle wages most fiercely, and here again the knowledge of structure is sought eagerly as the basis of explanation of the vital phenomena. So radical have been the changes in this direction that a new and complicated terminology has sprung up, and the simple undifferentiated bit of protoplasm has now its cytostome, cytolymph, caryosome, chromosome, and with their somacules and biophores. These accurate studies in the vital units have led to material modifications in the theory of descent."

It is unnecessary for us to trace fully the history of the idea of 'spontaneous generation,' which has persisted for twenty centuries. The story has been recommended 'to the psychological historian as a labyrinth of error, with glimpses of truth at every turn.'

Leeuwenhoek, by using a crude microscope, showed 'spontaneous generation' to be only apparent, not real. A memorable scientific battle was fought over parasitic animals. "Adam was said to have contained all the human parasites from the first—a state hardly consistent with Edenic bliss." Now *Darwinismus* makes such assumptions useless.

Pasteur almost proved that all 'life comes from preexisting life.' But Tyndall and Dallinger learned that in many cases 'young and immature germs could survive the boiling temperature, growing and propagating themselves when the liquid subsequently cooled.'

Life was in every case traced to other life. Its origin remained a profound mystery. Man beat in every direction hoping a door might open. Kelvin suggested that germ life may have been a meteoric passenger from otherwhere. Allowing such arrival gave no answer to the question as to the origin of the life found on the meteorite. Helmholtz, in advocacy of this 'cosmozoic hypothesis,' said, 'Organic life either came into existence at a certain period, or it is eternal.'

Just a few years ago Professor Rücker in his address before the British Association said: "Perhaps the chief objection which can be brought against physical theories is that they deal only with the inanimate side of nature and largely ignore the phenomena of life. It is, therefore, in this direction, if any, that a change of type may be expected." Before then the Count de Gasparin wrote in the *Journal des Débats*, 'Take care; the representations of the exact sciences are on their way to become the inquisitors of our days.'

Projected as we are upon the stream of life, we endeavor to learn first how it continues and thus reach its source. This is not an altogether illogical method; quite the contrary, as it rests upon the great principle of true inductive reasoning from experience to cause.

There are two great principles upon which the philosophy of nature now rests. They are the doctrines of the conservation of matter and the conservation of energy. These are dependent upon our conception of length and time, measured by arbitrary standards. A metallic bar is the former unit. The revolution of the earth gives us a day, which may be shorter or longer according to tidal friction. Or the recurring seasons give us the year unit, depending upon the course described by our relation to the sun. We are aware of necessary calendar changes as a result. And yet Cavendish's idea of science was measurement!

These tenets have recently been brought into question as a result of the investigations pursued in the laboratory where Newton, Clerk-Maxwell, Stokes, J. J. Thomson and Rutherford worked. Before the inter-

molecular phenomena are understood, the marvels of the intra-atomic physics have amazed a decade accustomed to wonders.

One form of matter may be converted into another. Accepting for the sake of argument that this transformation may even take place among the elements, and it is by no means necessary that we do, we may abide a while longer with the idea of the discontinuity of matter. Our atomic base may be shifted, which is just as well, but with our conscious powers and experimental observations, we are not yet ready to say that something is made up of nothing. Matter occupies space and has weight.

We may, therefore, for the time being disregard the metaphysical energism of Ostwald, to revert to it again. Through the basic studies of thermo-dynamics by Willard Gibbs we arrive at the equilibrium in physical and chemical systems. Lord Kelvin, considering the dissipation of energy, reached the logical conclusion that permanent equilibrium occurs only when availability is a minimum. A point I desire to make, particularly, here is that the limiting brain of man finites the infinite in his experiments or actual experience.

We know life only in its association with matter. Yet it is not matter, it occupies no space, has no weight, as we know gravity. The dead cat is under the same gravitational influence as before the loss of the traditional nine lives. A copper wire weighs the same whether the current pass or not. A bar of iron neither gains nor loses in mass when it is magnetized.

These latter can do work; they have energy. The analogy—a dangerous thing not alone in science—is striking.

We know no matter without energy. Do we know any energy except through its manifestation on matter?

The brilliant metaphysical teachings of Ostwald demand our utmost consideration, whether we give allegiance or not. In abstract, his reasoning asserts that we are aware of matter only through energy. Matter is an assemblage of energy systems; there is no matter. All resolves itself into the mechanics of energy. Life, electricity, heat are elementary energy systems, having definite capacity and intensity, as chemical entities with their equivalents represent our atomic conceptions.

Is life energy? We know of no manifestation of life without evidence of energy. It does not necessarily follow that all energy is accompanied by life demonstrations. I am not sure but we shall yet see that energy manifestations include life.

A stone rolling from an elevated position has kinetic energy by virtue of its motion. If its progress be checked by an obstacle in its path the translational energy is converted into heat energy. One form is converted into another. There is no loss of energy. If the obstacle be removed the body continues its descent. An impulse was necessary. At rest it possessed potential energy by reason of its position. To acquire that position equal muscular or other form of energy was necessary. There was no gain of energy. That depended upon the combustion of the tissues, a chemical process, chemical energy. We are accustomed to the idea that all energy may be measured either as gram-centimeters or as foot-pounds, merely convenient units into which other terms are easily reducible. Considering life from the energy point of view Hibbert says, 'All other forms of energy can be measured in foot-pounds of work.' Therefore, life is not energy.

I am not altogether sure that we have measured the energy of the Röntgen rays. The charge on the cathode particle was the recent fundamental determination guiding J. J. Thomson. Rutherford has just succeeded in learning the positive charge on the alpha-particle from radium. These

are forms of energy, the nature of which in part only has lately become known. Certainly it is suggestive that refinements may possibly in time give us measurements of a life-energy!

Energy activities are not seldom actuated, directed and facilitated by certain inorganic or lifeless things. These substances, which cause things to happen with acceleration, are known technically as catalytic agents. Sulphur dioxide and oxygen when mixed appear to remain so. The presence of platinum brings about a rapid union. Platinum and other finely divided metals convert alcohol into acetic acid, a process for a long time attributable only to the influence of living organisms.

In 1828 Wöhler bore down the barrier between inorganic or dead matter and organic substances, supposed to result solely through the activity of vital forces. It is now well known that certain complicated organic compounds of undetermined composition, as the enzymes, act as catalytic agents. Complex sugars are broken down into simpler sugars. Complex fats are built up from simpler constituents. Although the action may be attributed in part to chemical reactions, the details of the progressive and continuous action of these agents are as yet little understood and are demanding much attention at present.

When complex carbon compounds are burned with oxygen, heat is produced. The heat of this chemical action is simply the resultant of the energy absorbed and given out. We have so far secured no way of measuring absolutely chemical affinity, as we may measure heat in calories, for example. When certain organic bodies are mixed with oxygen without the presence of a living thing, or one of these exciters, no evidence of chemical energy, within reasonable time, has been noted. However, we are by no means sure that there are no energy manifestations. The exciter facilitates or accelerates its manifestation. The oxygen carried into the lungs reacts throughout the animal tissues. We are not yet ready to say how this is. It is not unfair to assert, however, that the energy binding the oxygen atoms together in the molecule is perhaps overcome in part through the organic catalytic agents present. We are aware of cases of suspended animation in which the organs returned to the performance of their functions.

The term catalyzer has been confined to substances and not used in reference to energy agencies alone. Grove considered the facts of catalysis dependent upon voltaic action, 'to generate which, three heterogeneous substances are always necessary.' We know that ultra-violet light will quicken the union of hydrogen and chlorine. By simply holding metallic tin at a definite temperature (20° C.) the whole crystalline structure is altered from tetragonal to rhombic. This does not occur at any other known temperature, requires no foreign substance, and we are unable to measure the actual energy involved. It is not unfair, therefore, to suggest that energy is converted under certain conditions into some transformation we have not yet learned to measure. Equally must it be true that under conditions this unmeasurable form of energy is transmuted into a measurable variety.

To the supporters of the doctrine of vital force, to quote Rücker again, "the principle of life was not a hidden directive power which could, perhaps, whisper an order that the floodgates of reservoirs of energy should now be opened and now closed, and could, at the most, work only under immutable conditions to which the living and the dead must alike submit. On the contrary, their vital force pervaded the organism in all its parts. It was an

active and energetic opponent of the laws of physics and chemistry. It maintained its own existence, not by obeying, but by defying them; and though destined to be finally overcome in the separate campaigns of which each individual living creature is the scene, yet like some guerilla chieftain it was defeated here only to reappear there with unabated confidence and apparently undiminished force."

We have arrived at the point where we may say that catalysators direct energy or facilitate its activity. So does life. There is much in common.

Going back to Ostwald, matter is an energy grouping. Catalytic agents then must be energy. Life is an agent of catalysis. In frankness, I am unable to conceive matter practically devoid of space occupation any more than I can comprehend energy except in the manner it is presented through its influence on matter. With or without the energetics of the distinguished German scholar, we may hold to the thesis, namely, life is energy or a manifestation thereof.

The experiments of Loeb and Matthews on parthenogenesis through the agency of dilute saline solutions strike at the root of the problem. According to the modern ideas of dilute solutions from the investigations of Faraday, Hittorf, Kohlrausch, Van't Hoff and Arrhenius the ions have electrical charges. Energy is involved, available. Böhn has recently effected similar reproduction through the influence of the rays of radium. These are forms of energy not yet measured.

One were devoid of judgment did he not let it be clearly understood that he appreciates the objections that may be raised, with reason, in opposition to the mechanical, physical, chemical or energy explanation of life. As yet we do not know the constitution of the highly complicated structures of the carbon, hydrogen, oxygen, nitrogen and sulphur compounds of the nucleus; 'chemical matter,' as Neumeister says. The same could have been truly said of the sugars before Fischer's masterly work beginning about a score of years ago. Can we say, having learned the structure, synthesized the nucleus, we shall not be able in the laboratory to give it that impulse which launches it upon a career of reproduction?

Powerful arguments favoring the vitalistic theory, consequently opposed to the energistic, are retention of form through years, reproduction of species and atavistic inheritance of character.

Considering these three in order, it becomes us to show satisfactorily their accordance.

1. Crystals beget like crystals in saturated solutions of the substance. Crystalline growth is by apposition. This is a most familiar phenomenon. Seed, one of the means of nature's reproduction, may remain years, centuries, in vaults, as within the Egyptian pyramids. When subjected to the proper conditions, they sprout and reproduce. Does it not appeal to reason to assume that the sprouting is just as well due to the renewal of the conditions favorable to sprouting, like moisture and proper temperature, as to some germ of life which may have remained dormant for all those years? All knowledge requires hypotheses. If the seed does not sprout then some factor is deficient. A fundamental law of chemistry, based on multitudinous experiments, is that like begets like. Monera, Haeckel's simplest protozoans, either the naked (gymnomers) or those with cell-walls (lepomera), grow by intussusception, or taking of new matter within their interior.

2. Substances to be absolutely the same must not fail in the least resemblance. No two acorns, no two oak trees, no two horses are ever exactly alike, although they may

have many similarities. The structure of the seed, cell or nucleus is complicated, else we should already have learned it. A simple concrete example will serve to illustrate our point. An equimolecular mixture of aluminum and potassium sulphates in water will crystallize in a definite form with a fixed proportion of water. By substituting ammonium for the potassium, we obtain crystals of the same form, with the same molecular proportion of water, yet they are different.

3. It is not difficult, in fact it is a common laboratory practice, to change several factors, either singly, alternately or simultaneously, so that the body first obtained resembles the parent in one or more ways or even not at all; yet on changing back one factor, the grandson resembles the grandfather.

The nucleus is made up of chemical molecules which are dissimilar. We know this much of the albumen molecule, namely, it is very large and complicated, containing from six hundred to a thousand or more of the nineteenth century atoms. They are combined in groups of variable sizes. We have to deal, therefore, with a system of several complicated components. There, doubtless, is a point at which such a system of definite composition may be held in perfect equilibrium through any length of time. If we add energy or take it away, as, for example, heat, the speed of the chemical reaction is altered. The reaction velocity is often reduced one half by a range of five degrees. One hundred degrees may cause it to fall to one millionth. The slightest change in medium, as adding or subtracting water, produces a marked acceleration or retardation in the speed as well as direction of the reaction.

It may be urged that when the nuclei containing these systems are subjected to certain influences, as heat or poisonous substances, they no longer germinate. The vital force was killed. We may also poison the dead catalyzing platinum and it is no longer active. These things are no more than we should expect. When a single factor of a complicated system has been changed, as readily happens through the agencies mentioned, we have no right to ask the same variation in the systems until exactly the same components become concerned in the former way. Ostwald puts it thus: 'As disturbances accumulate, the dissipative actions outweigh the accumulative ones, and the organism goes out of commission.'

With or without the energetics of Ostwald, the reasoning appeals to me. It offers a logical explanation of nature, which is growth. By adding, taking away or varying the components in any system, we may change from a simpler to a more complex system, or *vice versa*.

Bunge, discussing vitalism in physiologic processes, has most eloquently said, "Many centuries may pass over the human race, many a thinker's brow be furrowed, and many a giant worker be worn out, ere even the first step be taken towards the solution of this problem. And yet it is conceivable that a sudden flash of light may illumine the darkness." Science has no impossible boundaries. "Science will continue to ask and to answer even bolder questions. Nothing can stop its victorious career, not even the limitations of our intellect. This, too, is capable of being made more perfect. There is no rational ground for thinking that the continuous progression, development and ennoblement of type which has been going on for centuries on this planet, should come to an end with us. There was a time when the only living creatures were the infusoria floating in the primeval sea, and the time may come when a race may dominate the globe as superior to ourselves in intellectual faculties as we are to the infusoria."

Professor E. B. Wilson, the retiring president of the New York Academy of Sciences, has recently addressed that body upon 'The Problem of Development.' The paper reached me just three days ago. It contains most interesting information on the question of vitalism and mechanism. "In so far as development may be conceived as the outcome of an original material configuration in the nucleus, and a secondary configuration in the protoplasm, it may be conceived as a mechanical process."

This leaves unsolved, however, certain fundamental elements of the problem, for example, 'the manner and order in which the protoplasmic stuffs are formed and assume their characteristic configuration,' or 'how the wonderful phenomena of the regeneration of lost parts in the organism can be explained.' Advances have been made in the solution of the problem on the mechanical basis, hence Dr. Wilson asks, 'by what right does the vitalist demand that we shall adopt his hypothesis for the portions still unsolved?'

I am fully aware that sufficient experimental data have not been obtained to reduce the complicated phenomena to our familiar physico-chemical terms. But as a result of his work on the amphioxus and the dentalium, Professor Wilson remarks, 'It is possible, probable, that living bodies may be the arena of specific energies that exist nowhere else in nature.'

Our survey of the development of natural philosophy has forced upon us one fact, which we can not avoid. It is that man's knowledge of nature has been a growth, an evolution. Just as Francis Bacon thought, truth in science can only be obtained by progressive generalizations. This is true whether we accept the teaching of Darwin or the opposing atavism of de Vries. The means whereby we have gained that extended knowledge are too numerous even for enumeration here. I doubt not the ancient Greek philosophers would have had some merriment in their pity for him who might have suggested the existence of such a substance as the torch of Satan (phosphorus), which was exhibited at the continental courts of Europe.

I am almost overcome with the thought of what I may see, when I consider the immensity of the panorama presented to my venerable predecessors, who happily survive this day. Their half century of greater experience than mine brought them to the light of radium, the penetrating energy of Röntgen, and the phenomena of parthenogenesis. Nations whisper their wonders thousands of miles through the pulsations of energy, which gives life.

Acceptance of this philosophy does not preclude man's reaching a higher state of perfection; nor does it obviate man, as he is, being the highest type this world may ever know. It will depend entirely upon the factors in the systems. When equilibrium of energy has come about, none will be available and life, all life, inorganic as well as organic, will cease. Our world will have come to an end. The degradation will be as imperceptible as the growth. That which is and was returns to that which has been forever—the quiescent ocean of energy in equilibrium, the source and recipient of all life, which we are pleased to name as God. Creation's chorus is stopped, 'hid in death's dateless night.'

"Gone—all gone—like the light on the clouds at the close of day."

Charles Baskerville.
College of the City of New York.

INTERPRETATION OF A WATER EXAMINATION.

Interpretation of a water examination may be considered from two quite different points of view. It may mean the private

weighing of evidence by the investigator himself, a procedure which finds expression in his final opinion, or, it may be his attempt, often a desperate one, to make analytical data intelligible to an unscientific audience. The first is, of course, necessary and legitimate, the second is always of questionable policy, and frequently is an undeniable mistake. In former days, when 'standards' were still much in vogue, and when the dictum of Wanklyn, that such and such limits should not be passed in the several items of water analysis, it was indeed a difficult matter, for the analyst to escape from 'explaining' the analytical results to the assembled council of city fathers, and deep was the irritation felt by those people that the figures given could not be explained as clearly as they might have been were the case one involving the composition of an iron ore. Of course, those were times when the chemical data alone were considered sufficient whereon to formulate a pronouncement as to the quality of the water, and it is to be admitted that the chemist himself frequently found before him a very complex problem when he attempted to fit the results of his analysis to the sanitary facts known to relate to the water in question. Bacteriology was as yet undeveloped and its bearing upon the 'sanitary survey' had not as yet seen the light. A sample of water taken anyhow, in any kind of vessel and by anybody, was packed off to the chemist; all knowledge as to where it came from was intentionally withheld and a complete report of its sanitary qualities was confidently expected. The writer once received a sample of a town supply in a two-ounce Lubin extract bottle which still contained some of the original perfumery. Is it to be wondered at that in those early days a good share of discredit was cast upon a water examination? With the advent of bacteriology upon the scene, interest was greatly awakened. The new science promised much, and it seemed that the time had come for very positive and ready answers to the perplexing questions which had bothered us so long. Not so many years ago there met in the city of New York a sizable number of men who had gathered for the purpose of discussing the merits of a chemical *versus* a bacteriological examination of water. Advocates of the two methods advanced arguments in support of their special views and offered illustrations calculated to expose the weak points of their opponents. Unfortunately some remains of that spirit of rivalry still exist; but those who have the widest knowledge of the broad field of 'water supply' readily admit that a competent investigation suitable for determining as to the purity of a city's water service can not be undertaken in the laboratory of either the chemist or the bacteriologist or the microscopist alone, but must be the product of a draft upon the sciences represented by all three of those men, and must, furthermore, include the findings derived from what may be termed the 'sanitary survey.' I am speaking to scientific men, who need no instruction, but perhaps over their heads a few laymen may be reached who need it sadly. And now let it be asked, who are to be classed as the laymen? There is but one answer, to wit, all who have not given special study to this particular subject. The field is so wide, is increasing at so rapid a pace and covers such variety of topics, that even those interested in this line of work have all they can do to keep in touch with the changes taking place about them. The writer well remembers the great risk he once ran of making himself unpopular with the medical profession. While addressing a city council upon the advisability of erecting a special form of filter for the purification of the public water supply, he was interrupted by a council-

man, who stated that all of the physicians in the city were opposed to such a plant, and therefore how could the council, composed, as they were, of laymen, run counter to such weight of professional opinion. The answer was simple and emphatic, namely, that upon such a subject the physicians were no less laymen than were the councilmen themselves, and that the paper prepared by them was of no greater worth than it would have been had it been signed by all the lawyers or all the clergymen of the town.

Referring to what has been touched upon above, it is a mistake to underestimate the value of the 'sanitary survey,' by which we mean a thorough knowledge of the source whence a water comes and of the opportunities for pollution, both constant and occasional, to which it may be exposed. In the writer's judgment it is not too much to say that if but one form of examination be possible, the 'sanitary survey' should be the one selected.

Then why not rest satisfied with such examination and permanently exclude chemistry and bacteriology from water cases; and why is not the city engineer an authority competent to express final judgment upon the matter in hand?

In reply it may be said that because of the greatly increased public interest in 'water supply' which has develeped during recent years, there has arisen a class of men who have devoted nearly their whole time to the consideration of water questions and who have brought to their aid a sufficiency of chemistry, bacteriology and microscopy to satisfy the requirements of their calling. Such men are, because of their special training and experience, enabled to view the question from more than one side, and their conclusions have, in consequence, greater scope.

Although the writer believes that, taken alone, the 'sanitary survey' is, in the majority of cases, the most important form of examination, he begs not to be misunderstood.

No amount of inspection could be substituted for the bacterial count in testing the efficiency of a filter plant, nor would it be of value in warding off danger to a ground water arising from the presence of an unsuspected cesspool.

As showing the utility of the chemical examination, take the following instance for example:

A well which was most highly prized because of the cool, pleasing taste of its water was found loaded with chlorides and nitrates. Bacteriology gave no indication of pollution, and inspection of the surroundings was spurred into energy by the chemical results alone. Sewage, completely oxidized, from neighboring vaults was found to account for the abnormal items in the analytical results. At the time of the examinaticn no harm was being done, but would the owner of the well be justified in continuing to use such a water and take his chances of the purifying action of the soil being always effective?

It is possible that some objection may be raised to the condemning of a water which shows as its only objectionable feature a chemical evidence of 'past pollution.' If the pollution be truly past and all of the nitrogenous organic matter be represented by nitric nitrogen; and, further, if bacterial examination result favorably, then wherein lies the objection to the use of a water which, although once polluted, has regained its potable qualities? All pure waters, it may be contended, might be classed under such a head; for, after all, we are bound to use water over again sooner or later, contrive matters how we may. All this is true enough, but there is surely a preference as to the length of time between the date of present use and the period of 'past pollution.'

It is true that every time we drink filtered river water we are imbibing a purified sewage of greater or less concentration, and, with a continued growth of our great cities, and the increased pollution of our water-sheds, it would seem that the day is not far distant when a naturally 'safe and suitable' water shall become a thing of the past, and we shall be forced to employ a purified water as our only source of supply.

Let it be remembered, however, that we can control the artificial purifying devices of which we make use, and we can repair them, should they at any time refuse their work.

The case is quite different, however, when our safety lies upon the proper operation of those natural processes of purification which are beyond our power to direct. Such purification, to be satisfactory, must appeal to us as being continuously effective.

We know very well that the raising of water vapor by solar heat will leave objectionable material behind, and we are satisfied that the result is perfect and that it will continue to be so during all time. We also know that the filtering and oxidizing power of the soil is very great, and in general we are willing to pin our faith upon its efficiency. But we can not avoid a feeling of uncomfortable doubt when we note that a small amount of soil has been given a large quantity of work to perform, and we naturally ask, can not the purifying powers of such soil be overtaxed, with the result that our protective filter will become damaged at a point beyond reach of repair? Let an English case be quoted here:

A certain farm-house was notoriously unhealthy. The inmates had suffered at various times from diphtheria and typhoid fever. The water had been examined, and was reported to be satisfactory. Upon examining the premises it was found that there was a water-closet in the house, which was in good order, but where the contents were discharged was unknown. The drains were said to be satisfactory and never to get blocked, and upon tracing them, it was found that they discharged into a dry-steyned cesspool without overflow about four yards from the well, both sunk in the gravel, which here was twenty feet or more in thickness. This well yielded an unfailing supply of water, which was used for all domestic purposes, and upon analysis it was found to be remarkably free from organic matter. It was said to be always cool, bright and sparkling, probably due to its containing a very excessive amount of chlorides and nitrates derived from the sewage percolating into the subsoil and the opinion was expressed that the water was a concentrated purified sewage. This was not believed at the time, but when the cesspool was filled in and the sewage carried elsewhere, the well ran dry. There is no doubt that in this case the same water was used over and over again. After being defiled by the closet, slops, etc., it ran into the cesspool, then filtered through the soil, in its progress the organic matters becoming completely oxidized, and ultimately it found its way back to the well, to be utilized again for domestic purposes. Doubtless at times, possibly after heavy rains, the cesspool contents filtered too rapidly for complete purification to be effected, and this impure water may have been the cause of the ill health amongst those who consumed it.

In this instance, as in the one quoted by the author, the danger signal was held out by the chemical side of the investigation alone, the other methods of inquiry failing to detect any trace of evil.

It would seem that bacteriology deals with the present and that chemistry, besides throwing light upon the past, does to some degree, prophesy what may happen in the future.

Many a water which the bacteriologist has pronounced harmless has been condemned by the chemist because of what it might unexpectedly become at some future time; and, on the other hand, the bacteriologist has time and again shown the presence of unlooked-for pollution when the chemist might search for it in vain.

A good instance of the saving of the situation by a 'sanitary survey' when both chemistry and bacteriology show adverse

reports is to be found in the examination of water from a new well or a recently 'developed' spring. Given an old and well-situated spring upon a hillside, the desire of the owner to 'improve' the property with a view of placing the water upon the market will commonly result in a disturbance of the immediately surrounding soil. From a sanitary outlook no harm has been done the water, and one familiar with the situation will offer no objection to continuing its use, but both the chemist and the bacteriologist will secure analytical results which will require to be explained to avoid an adverse report. The writer has seen many cases of this kind.

Wells which are newly dug likewise furnish water of temporary apparent pollution. Distinction must here be made to allow for actual pollution arising from foreign substances being left at the bottom of the finished well. In such instances the evidence pointing to contamination will be found to persist.

The tying up of pollution through the action of frost is another fruitful source of error, if the judgment be controlled by the laboratory data alone. Swamp waters commonly improve in winter, and samples of them will mislead the analyst who is unfamiliar with the districts whence they come. Again, the same agency will solidify surface sources of contamination like those which produced such havoc at Plymouth and New Haven, and the laboratory examination, whether chemical or bacteriological, will, throughout a northern winter, utter no prophecy of what is to be expected during the coming thaws of spring. Nothing short of a thorough sanitary survey can be depended upon in such instances.

The water in a tidal river may be unimpeachable during ebb flow and quite the reverse at periods of flood. How could an analytical examination at the former stage of the stream predict what might be expected at change of tide.

Instances very often arise when public clamor is heard loudly complaining of the taste and smell of water supplied to the people. Much irritation is felt whenever the senses are offended by its physical condition, although gross pollution by pathogenic organisms will be complacently accepted. This tendency of the public to be their own judge as to the suitability and safety of the water they are asked to drink reminds one of the decision of a Mississippi court in a case with which the writer had to do about a year ago. His honor said: "It is not necessary to weigh with tenderness and care the testimony of experts. An ordinary mortal knows whether water is fit to drink and use."

Would that the ordinary mortal did know. Typhoid fever might then be relegated to the list of rare diseases, and much money and many precious lives be saved.

When odors in water occur, what is the analyst to do? By the time the laboratory is reached all smell may have left the sample and great discredit of the scientist will follow should his statement be that the water is sound, when the users thereof know to their sorrow that something is the matter with it. An examination *in situ* is what is needed in cases of this sort, and a view of the storage reservoir backed by microscopic detection of the offending organisms will do vastly more good than any amount of chemical analysis.

A man now deals with the data of water examination in a broad-gauged fashion, feeling that the day has gone by for blind adherence to cut and dried standards. He approaches his decision pretty much as does the medical practitioner frame his diagnosis at the bedside. It may be that the symptoms of the patient do not accord with the description of the disease as found in the books, and the practitioner's atten-

tion may be called to those discrepancies by a coadjutor more recently from the schools, nevertheless the breadth of his experience assures the more mature man that his judgment is not at fault and it is experience that is of value in the end.

In conclusion, a word may be pardoned concerning a matter which has received more or less attention of late from the public press, namely, the treatment of reservoir water with copper sulphate for the purpose of destroying suspended organisms. No doubt whatever exists that a sufficiency of the salt will destroy aquatic life, and the amount required to dispose of such as produce objectionable taste and smell is certainly very small.

What the public are anxious about, however, is whether or not the salts of copper are to be classed with those of lead as cumulative poisons. Unfortunately, the answer to that question is not very satisfactory at the present moment. We do not possess as much light upon the point as we should wish.

Copper is eliminated by the liver and kidneys, and some hold that there is a tendency towards an accumulation of the metal in the liver, and that 'elimination is only complete when eliminating organs are sound.' This appears reasonable. On the other hand, we should be reminded that the use of copper sulphate for preventing algal growth is but occasional, and that no necessity is at hand for asking the people to constantly use a water treated with the salt.

A dose of the chemical is administered to the reservoir water; the objectionable plants are killed thereby and no further dosing is required during a considerable interval of time. Let it be noted, therefore, that the amount of copper used is minute, that all of it does not remain in solution, and that its use is not continuous.

As to the employment of copper sulphate for the killing of pathogenic bacteria the case is quite different. Under such conditions the amount of the sulphate required has to be greatly increased, and, what is still more objectionable, its addition to the water supply must be constant, because of the continual presence of the organisms which require removal. It may well be urged that the use of a 'disinfected' water supply would be opposed by the average citizen upon pretty much the same ground that he would object to the use of embalmed beef.

Some modification of the copper process for the killing of disease germs may yet be suggested which will excite the prejudice in the popular mind against 'chemicals' to no greater degree than does the employment of alum in mechanical filtration, but that day is scarcely here as yet. Let it not be forgotten, however, that its use for removal of those algal growths which have given us so much trouble in the past is to be encouraged, and that the authors of the process are deserving of much praise for their contribution to the growing field of 'water supply.'

W. P. MASON.

SCIENTIFIC BOOKS.

A NEW INTRODUCTION TO THE STUDY OF FISHES.

I.

A FULL fourth of a century had passed since the publication of a general work in English* on systematic ichthyology before a new one appeared to take its place. It was in 1880 that 'An Introduction to the Study of Fishes by Albert C. L. G. Günther' appeared. That work, however, by no means represented the condition of science at the time of its issue, and was replete with errors as well as anachronisms of all kinds. Its author was

* E. Perrier's corresponding portions of his French work (Traité de Zoologie) were mostly published less than a year before (1903), and, if put in the same typographical dress, would cover nearly two fifths more space.

then the custodian of the fish collection of the British Museum as well as 'keeper of the zoological department.' His successor as custodian of the fishes, Dr. George A. Boulenger, is one of the authors of a new work covering practically the same ground as Dr. Günther's. The new work labors under the disadvantage of having no real descriptive title-page. On a bastard title-page it is designated as 'The Cambridge Natural History—Volume VII.,' and on the true title-page it has the following legends apportioned and punctuated as here represented.

HEMICHORDATA
By S. F. Harmer, [etc.].
ASCIDIANS AND AMPHIOXUS
By W. A. Herdman, [etc.].
FISHES (Exclusive of the Systematic Account of Teleostei)
By T. W. Bridge, [etc.].
FISHES (Systematic Account of the Teleostei)
By G. A. Boulenger, [etc.].
London
Macmillan and Co., Limited
New York: The Macmillan Company
1904

We are thus compelled to refer to it as the Cambridge Natural History, Volume VII.

The new work, in line with modern concepts respecting the vertebrates or chordates, includes not only the lower types of the vertebrates of the old naturalists, but also the Hemichordata and Urochordata or Tunicates. The old class of fishes of the 'Introduction' is replaced by the three classes for more than a generation past adopted in America, that is, the 'Cephalochordata' (Leptocardians), the 'Cyclostomata' (Marsipobranchs) and the 'Pisces' (Teleostomes or fishes proper).

It may be noted that the names Hemichordata, Urochordata and Cephalochordata are given as terms of subphyla and not as class names. The constituents of the first, for Dr. Harmer, are the 'orders' Enteropneusta, Pterobranchia and Phoronidea, each of which has been considered by some as a class, or, at least, far removed from the others; the second is universally known as the class of Tunicates or Ascidians, the third as the class Leptocardians. The three subphyla thus named are succeeded by another subphylum—'IV. Craniata,' which is divided into two classes: 'Class Cyclostomata,' generally called Marsipobranchs or Myzonts, and 'Class Pisces,' including the Selachians and true fishes, or Teleostomes.

II.

The three 'orders' aggregated as 'Hemichordata' can not be considered to have been proved beyond all cavil either to be closely related or to be true Chordata. The student may find a summary of the arguments respecting the 'affinities of the Hemichordata' at the end of the chapter on the group (pp. 30–32). It is not long since almost all the known species of Enteropneusta were supposed to be referable to one genus—*Balanoglossus*. Now they are distributed among three families and the oldest of them appears under the guise of Ptychoderidæ.

III.

The 'Urochordata' or Tunicata have been elaborated in excellent style by the eminent monographer of the 'class' (Professor Herdman), who has long been known in connection with those animals. In spite of the many different changes and systems that have been proposed by others while he has been actively engaged on the group, he retains practically unchanged the system he employed in the Encyclopædia Britannica (1888) and the *Journal of the Linnæan Society* (1891). It is noteworthy, too, that the name *Cynthia* is still kept, although there is a well-known genus of Fabricius (1808) so termed long before Savigny's genus (1827) was established. That the retention was deliberate and in spite of the facts is evident from a note to the same in the *Journal of the Linnæan Society* (23, 576), where the substitute '*Halocynthia*, Verrill, is [declared to be] merely a synonym.'

IV.

The main structural features of the 'Cephalochordata' are passed under review in an able manner and the latest sources of information made use of. The classification is derived by Professor Herdman from Mr. Walter Tattersall; that author is evidently

well informed, but his logical faculty and taxonomic tact will be disputed by some at least.

The 'sixteen species' recognized are grouped under two genera—*Branchiostoma*, 'having biserial gonads and symmetrical metapleura' and *Asymmetron* 'with uniserial (right) gonads and asymmetrical metapleura.' One of the species referred to *Branchiostoma* is the *B. pelagicum.* According to the original describer 'buccal tentacles are absent,' and this statement has been corroborated by all subsequent observers; the last examiner of the species (G. H. Parker, in November, 1904) had an 'exceptionally well preserved specimen' and could 'confirm the statement of most previous writers that oral cirri are absent.' Parker was also, like C. F. Cooper, 'unable to find any evidence of branchial apparatus.' Furthermore, the gonads, though in two rows, 'are often so closely pressed together near the median plane that they there seem to form a single median row.' Surely a species distinguished by such trenchant characters and also distinguished by its pelagic life is entitled to distinction from all its fellows! Owen and the old naturalists generally considered the development of the mouth as 'a longitudinal fissure with subrigid cirri on each side' to be an *ordinal* character of the '*Cirrostomi*' named for *Branchiostoma.* Unquestionably the character is of *generic* value at least, and the form differing so decidedly from it may be ranked not only as the type of a distinct genus (*Amphioxides*), but distinct family (Amphioxididæ). The details of the oral structure, however, remain to be made known.

A flagrant violation of a principle of nomenclature adopted by all learned societies may next be noticed. The name *Asymmetron* has been adopted for all the species 'with uniserial gonads.' Now, *Asymmetron* was not named till 1893, and long before (1876) Peters had named a genus *Epigonichthys* for a species which is believed by the author to be congeneric with *Asymmetron.* If such were the case, the prior name, *Epigonichthys*, should of course have been used for the genus. As a matter of fact, however, some naturalists at least will adopt the names *Epigonichthys* and *Asymmetron*, as well as *Paramphioxus*, for special species or groups of species thrown together in *Asymmetron.* It may be added that the fact that Peters did not appreciate the proper generic characters is not a necessary corollary of the question at issue; he gave the name in connection with an undoubted species and tried to define it.

V.

The 'Craniata,' or rather the pisciform craniates, of course are the chief subjects of the volume, the 'craniata' being equivalent to all the vertebrates of the old naturalists before the recognition of the Branchiostomids. These are considered under two classes, (1) the Cyclostomata and (2) the Pisces. The former, and of the latter the Elasmobranchii or Selachians, and the Ganoids of the Müllerian system, have been treated by Professor Bridge; the Teleosts are summarized by Dr. Boulenger. The elaborate chapters on the anatomical systems and organs are also by Professor Bridge.

On the whole, the chapters on anatomy and physiology are apt and as full as could be reasonably expected in a volume of the series for which it was prepared. That on 'the skeleton' (Chapter VIII.), however, is insufficient in view of the extreme importance of the various osseous elements in the determination of the relationships of all fishes. All the non-teleost fishes might be lost and their loss made good, numerically, by the discoveries of a single year, yet all the space that is devoted to the skeletology of a teleost fish is less than ten pages (pp. 205, 211–216, 237, 240, 246); the species selected, the trout, is also not typical, a far better representative being the one chosen very many years ago by Cuvier and retained by Günther—the perch. The nomenclature of the bones is that current for a number of years past in Europe. Long ago, however, Sagemehl expressed doubt whether a single bone of the fish's skull was really a homologue of any in the terrestrial vertebrates. We fully share in that doubt, or rather belief, but for the present may retain the time-honored names derived from mammalian anatomy for the fish's bones. We can

not, however, do so for the elements of the shoulder-girdle; the bones of the trout for which the names scapula and coracoid have been used (p. 240) can not possibly be the homologues of the bones so called in the mammals. The fish bones only became developed as independent bones in those fishes which had originated from a holostean stock and when later specialization in a direction toward the Acanthopterygian type had supervened. To call such bones scapula and coracoid is to inculcate a most misleading concept of piscine morphology and development.

Attention may be called to another statement whose ambiguity will mislead. It is said (p. 252) that 'in some of the wrasses (*Labrus*), the inferior [pharyngeal] teeth are opposed to superior teeth on the upper pharyngeal bones'; experiments show that this sentence may be interpreted to mean that teeth on the superior pharyngeals are exceptional, whereas they are there, as a rule, not only in all Labrids, but in Acanthopterygian and many other fishes. Professor Bridge doubtless knows better, but has been unhappy in the use of words.

Another phase that may perplex the student is the frequent incongruity between the names of fishes referred to in the anatomical chapters and those adopted in the systematic portion of the work, such as *Mesoprion* (p. 235) for *Lutjanus* (p. 663), *Lutodeira* (p. 256) for *Chanos* (p. 294), *Rhombus* (p. 275) for *Psetta* (p. 687), and the like. Such are simple enough, but there are some names which Professor Bridge evidently introduced without knowing what forms were involved. In one place (p. 262), the statement is made that "ciliated epithelium has been found in the intestine of a few species (*e. g.*, *Rhombus aculeatus* and *Syngnathus acus*)." The *Rhombus* here is by no means the same as the *Rhombus* elsewhere, but the *Stromateus aculeatus*. In a second place (p. 275), it is said that 'in *Labrus labrax* there are about sixty' pyloric cæca: now no *Labrus* nor labrid has any cæca and the bass (*Labrax* of Cuvier) has only about five; consequently some other explanation must be found.* Perhaps the statement was based on some hexagrammid, called by Pallas *Labrax*, which has numerous cæca. In a third place (p. 357), we are told that 'stridulating sounds may also be produced by the friction of the upper and lower pharyngeal teeth, as in a species of mackerel (*Scomber brachyurus*)'; there is no mackerel so named, but the fish meant is the 'common horse mackerel (*Caranx trachurus*)' whose relatives are mentioned elsewhere by Professor Bridge (p. 363) as 'horse mackerels (*Caranx hippos*),' and which represent a peculiar family —the Carangidæ (p. 677).

In 'An Introduction to the Study of Fishes' (p. 177), the law was dogmatically declared that 'with regard to size, it appears that *in all* teleosteous fishes the female is larger than the male; in many cyprinodonts the male may be only one sixth or even less of the bulk of the female.' In 'The Cambridge Natural History' (p. 413) it is correctly stated that, 'as a rule, in fishes females are more numerous than males, and *generally* they are larger, but to both statements there are notable exceptions.' It is noteworthy that in the very family of Cyprinodonts of which the males were declared to be very much smaller than the females, there is at least one notable exception in the case of the genus *Mollienisia* (*Mollienia?*), whose males are much larger than the females. Furthermore, the males contrast with the females in brilliant coloration and especially in the greatly expanded dorsal fin. Some other fishes whose males are larger than the females belong to the families Callionymidæ, Gobiesocidæ, Labridæ, Gobiidæ, etc.

In almost all cases, so far as known, the larger size of the male is coordinated with brilliancy of coloration or some other secondary character. In short, the rule seems to be that when the males are brilliantly colored or have other marked secondary characters they are larger than the females.

"The only examples of viviparous fishes,"

* Neither the origin nor cause of the strange confusion of names of two unrelated genera into factitious species has been indicated by Professor Bridge.

it is claimed, "occur in certain families of elasmobranchs, and in five families of teleosts, viz., the Blenniidæ, the Cyprinodontidæ, the Scorpænidæ, the Comephoridæ and the Embiotocidæ" (p. 418). To this list should be especially added the remarkable Zoarcidæ and Brotulidæ combined in a single family under the name Zoarcidæ in the systematic part of the work (pp. 712, 713). Some fresh-water 'Scombresocidæ' of the genus *Zenarchopterus* are also viviparous (p. 638). Certain Cyprinidæ (p. 584), Siluridæ and Cottidæ have also been declared to be viviparous, but the claims have not been quite fully proved.

VI.

The Cyclostomata are ranked as a class, and the two principal divisions, called 'Myxinoides' and 'Petromyzontes,' are designated as 'orders.' Why the old names Marsipobranchii, Hyperotreti, and Hyperoartia should be abandoned is not obvious. The groups, however, are valued in accordance with general current usage, but the last two are of at least subclass rather than ordinal value. The immense gap between the 'Myxinoides' and 'Petromyzontes' is apparently scarcely appreciated by most naturalists, but it was recognized by Ray Lankester a generation ago (1877) in his distinction of the two groups as *classes*. The differences are fundamental and affect all parts and organs. If, for example, Professor Bridge had presented a figure of the auditory organs of *Petromyzon* to compare with those of *Myxine* and other types (p. 388), the contrast could not fail to strike the observer with the requisite knowledge of comparative anatomy to judge of the facts.

The 'Myxinoides' are divided into two families—'Myxinidæ' and 'Bdellostomatidæ.'

The 'Petromyzontes' are aggregated in a single family, as usual called 'Petromyzontidæ.' (It should be Petromyzonidæ.) Professor Bridge evidently had an imperfect knowledge of the species. He mentions (p. 426) *Petromyzon* with 'three species widely distributed in Europe,' and just afterwards states that '*Ichthyomyzon, Bathymyzon, Entersphenus* [= *Entosphenus*] and *Lampetra* are also northern forms.' *Lampetra* was originally distinguished for two of the 'three species widely distributed in Europe.' The genera mentioned fall into two primary groups: *Petromyzon, Bathymyzon* and *Ichthyomyzon* in one, *Lampetra* and *Entosphenus* in the other.

The statement is made that 'a new genus and species from Chili has been recently described under the name of *Macrophthalmia chilensis*.' The supposed new type was later (1902) shown by its author (L. Plate) to be simply a stage ('Jugendstadium') in the development of the *Geotria chilensis*. The Petromyzonids of the southern hemisphere differ remarkably from those of the northern in their development as well as otherwise.

VII.

The Elasmobranchii are treated in the old-fashioned style. After the extinct orders Pleuropterygii (p. 436), Ichthyotomi (p. 438) and Acanthodei (p. 440), the now extant types are considered under the orders Plagiostomi (p. 442) and Holocephali (p. 466), and the Plagiostomi are, as of old, divided directly into Selachii and Batoidei (p. 457). Such a division is certainly not expressive of the facts of morphology. There can be no question that the structural differences between the so-called Notidanidæ and Chlamydoselachidæ, on the one hand, and all the other Selachians, are of much more morphological significance than those between the sawfishes of the families Pristiophoridæ and Pristidæ. The Heterodontidæ also appear to be widely differentiated from the others, though not as much so as might be inferred from the old-time allusions to them. In fine, the segregation into (1) Diplospondyli or Opistharthri (Notidanidæ), (2) Prosarthri (Heterodontidæ), (3) Tectospondyli (sharks without anal and rays) and (4) Asterospondyli (other living sharks) appears to comport best with structural and developmental facts as well as with the paleontological record.

The name Notidanidæ has been used just because it is the term employed by Professor Bridge, but it should be discarded. Professor Bridge, apparently, is content to take a name as he finds it without caring whether it is

justified by history or not. A number of the names adopted by him would be discarded by those who were willing to obey the codes of nomenclature formed by naturalists whose large experience had convinced them of the necessity of adherence to rules. The persistent violator of such rules places an obstacle in the path of the zoological student and helps to prolong unhappy discord and difficulty. Too often, however, he assumes the attitude of the wolf to the lamb! Examples of family names wrongly used are Notidanidæ (for Hexanchidæ), Scylliidæ (for Scylliorhinidæ), Spinacidæ (for Squalidæ) and Trygonidæ (for Dasybatidæ). The Linnæan name *Squalus* is not used at all and the name *Læmargus*, though at the same time much younger than *Somniosus*, and long preoccupied, is still retained. Of course it may be said nomenclature is of trivial importance and too much has been made of it, but if so why should those who regard it from such a standpoint be obstinate in ignoring rules when adherence need not affect them while it does others?

There is, too, sometimes inconsistency between Professor Bridge's definition of a group and its contents. The family Lamnidæ is said to be composed of "large stout-bodied sharks with two dorsal fins, the first just behind the pectoral fins, the second, which is small, opposite the small anal fin; * * *. Tail with a prominent lateral keel on each side. * * * Branchial clefts very wide." It would thus appear as if Professor Bridge had adopted the family with the same limits that had been given to it by Müller and Henle and American ichthyologists. On looking at its contents, however, it appears that the genera for which the families Odontaspididæ (or Carchariidæ) and Alopiidæ have been framed are referred to it. Yet *Odontaspis* certainly has not the first dorsal 'just behind the pectoral fins,' nor the second or anal 'small' (but unusually large), nor 'the tail with a prominent lateral keel.' Nor does *Alopecias* (properly *Alopias*) agree better. That genus has not the first dorsal 'just behind the pectoral fins,' nor 'the tail with a prominent lateral keel,' nor the 'branchial clefts very wide.' As Professor Bridge had recognized the importance of the differentiating characters in the diagnosis of the Lamnidæ, he should have recognized the families Odontaspididæ (or Carchariidæ) and Alopiidæ by name.

A word may be in place as to *Alopias* and *Alopecias*. It is true that *Alopecias* was the ancient Greek name of the thresher, but Rafinesque thought it was too long and preferred to give a new name to the genus (as he had a perfect right to do); he selected *Alopias*, which can be perfectly and legitimately formed from ἀλωπος and the suffix *-ias*. Müller and Henle first substituted *Alopecias*, but in their great work reverted to *Alopias*: *Alopias* it should be.

Another notable case of inconsistency is manifest in the treatment of the family 'Scylliidæ' (Scylliorhinidæ). That family is defined as being 'oviparous,' having 'egg-cases large, quadrate,' etc. (p. 446). To it are referred '*Chiloscyllium*,' 'a widely distributed genus,' and *Crossorhinus* (*Orectolobus*). Yet both of those genera were shown in 1901, by Edgar R. Waite, to be ovoviviparous, like most selachians, and referred to distinct families, the Hemiscylliidæ and Orectolobidæ.

VIII.

The non-teleost 'Teleostomi' are disposed of in a somewhat peculiar manner. In the group are included the 'order I. Crossopterygii' (p. 476), 'order II. Chondrostei (Acipenseroidei)' (p. 485), and 'order III. Holostei (Lepidosteoidei)' (p. 495) and from it are excluded the 'subclass III. Dipneusti (Dipnoi)' (p. 505). It appears to be more than problematical whether such an arrangement is the best expression of the present state of our knowledge of the fishes involved. The relationship of the primitive Crossopterygii and Dipneusti was so close that they were confounded in one and the same group (suborder Ganoidei crossopterygidæ) by Huxley, and the gap between the two appears to be much less than that between the Crossopterygii and the nearest related of the existing fishes. Further dissent need not be expressed here. It may be recalled, however, because the discovery is so recent, that George

Wagner (1904)* has recorded the presence of a scaly zone behind the gill cavity, as well as the existence of a pair of minute barbels in *Polyodon,* thus falsifying the characters body 'apparently scaleless' and 'barbels absent' attributed to the family Polyodontidæ by Bridge.

The Teleostei, including almost all the living fishes, have been classified by Boulenger, and the work is worthy of that master of taxonomy and verbal expression of relationships. The group, formerly and generally designated as a subclass, is degraded to ordinal rank, and all the chief divisions, mostly called orders by American and some other ichthyologists, are designated suborders. Of the suborders there are eleven.

A large number of the groups familiar to American ichthyologists are accepted with practically the same limits as are current in the United States, but always with the inferior rank indicated, the orders being designated by Boulenger as 'suborders.' Such are the (1) Malacopterygii, (3) Symbranchii, (4) Apodes, (9) Anacanthini, (10) Acanthopterygii, (11) Opisthomi, (12) Pediculati and (13) Plectognathi. Other suborders have received families which had been ejected from other groups, the suborders thus enlarged being the (5) Haplomi, (6) Heteromi and (8) Percesoces. Another suborder (2. Ostariophysi) has been made to include the Nematognathi and Plectospondyli, the main difference from American practise being in the fusion of the groups, for the relations of the constituents of the so-called 'suborder' have long been recognized, as has the group itself as a 'superorder.'

How divergent this arrangement is from that long adopted in Europe is told by Boulenger (p. 543). "In the classification of Günther, which has been generally in use in [England] for the last thirty years, the Teleosts were divided into six principal groups, of ordinal rank: I. Acanthopterygii; II. Acanthopterygii Pharyngognathi; III. Anacanthini; IV. Physostomi; V. Lophobranchii; VI. Plectognathi. Group [order] I. corresponds to sub-order 6 (part), 7 (part), 8 (part), 10 (part), 11 and 12 of the present work; Group II. to sub-order 10 (part); Group III. to sub-order 9 and 10 (part); Group IV. to sub-order 1, 2, 3, 4, 5, 6 (part), and 8 (part); Group V. to sub-order 7 (part); and Group VI. to sub-order 13.

Some of the modifications introduced into the system are rather startling. The Murænidæ, we are told, differ from the other Apodes in that their dentigerous bones are the palato-pterygoid, the maxillaries being absent, while in the Anguillidæ and others the dentigerous bones are the maxillaries. It is difficult to believe that the dentigerous bones, specialized as they are, should be so different homologically. Exception must also be taken to the reference of the genus *Derichthys* to the family Anguillidæ. That genus has both intermaxillaries and maxillaries, and if it must, perforce, be referred to some former order, it is with the Symbranchii and by no means the Apodes that it should be associated. Anyway, it is the representative of a very distinct family—Derichthyidæ. Another group whose new allocation we can not assent to is the Saccopharyngidæ. The fishes of that family differ markedly from the true Apodes by the absolute want of all opercular and branchiostegal as well as various other bones, and, indeed, have no similarity, except in elongation of body, to the eels. They are more likely to be divergents from some stomiiform stock. From all other fishes, however, they are widely differentiated, and well entitled to rank as the equivalent at least of the suborders of Boulenger—the Lyomeri.

The Comephoridæ are extended to embrace, besides *Comephorus,* the recently described *Cottocomephorus* as well as *Anoplopoma* and *Triglopsis.* Dr. Boulenger expresses the opinion (p. 697) that 'no doubt can be entertained as to the propriety of referring [*Comephorus*] to the neighborhood of *Anoplopoma,*' but after a careful comparison of specimens the reviewer is unable to appreciate a resemblance sufficient to entail approximation in the same family. We may well avail ourselves of the technical character admitted by Boulenger himself; in *Comephorus,* 'the second suborbital is *not produced* over the cheek, a unique exception to the main

* SCIENCE, XIX., pp. 554, 555, April 1, 1904.

characteristic of this division,' and this alone will permit us to keep *Anoplopoma* apart as the type of a distinct family, Anoplopomidæ (or Anoplopomatidæ, if you will), since in this genus the suborbital bone is 'prolonged over the cheek towards the præoperculum' (p. 693).

Triglopsis is unquestionably a typical Cottid and scarcely distinguishable generically from the common *Cottus* or *Oncocottus quadricornis*.

If as to these (and a number of other groups) we must agree to differ, it is gratifying to find that such a self-reliant investigator as Dr. Boulenger, who would rather differ than not, has independently reached the same conclusions as American naturalists in many cases, and has correspondingly abandoned the views so long current in Europe. For example, he has recognized the distinctions and mutual relations of the families of Hemibranchii, Scleroparei, Pediculati and Plectognathi, or at least most of them, which were so long denied by the Güntherian school. There is, too, a notable agreement or approximation to agreement in very many other respects.

The recognition of the high rank of the Discocephali is also a triumph of reason over prejudice and leadership. Its type, *Echeneis*, was declared by Dr. Günther in 'The Introduction' (p. 460) to be closely allied to 'the genus *Elacate*, from which it differs *only* by the transformation of the spinous dorsal fin into a sucking organ'! Gill, after a study of the skeleton (1883) declared that *Echeneis* 'differs *in toto* from *Elacate*' and revived a name given long before by Bleeker. Nevertheless, a man who gave some consideration to osteological characters (F. A. Smitt), in his 'Scandinavian Fishes' (p. 89), thought the 'genus may still lay claim to a place among the Scombridæ, though the family-diagnosis can scarcely notice all such variations'! Boulenger is willing to be influenced by the characters and, therefore, remarks (p. 691) that, 'in spite of a superficial external resemblance to the genus *Elacate*, the sucking-fish bear certainly no affinity to that genus nor to other Scombriformes, as first observed by Gill.'

There is the usual statement (p. 593) in ichthyological works, that the 'only European representative of the family' Siluridæ is the *Silurus glanis*. Over twenty-two centuries ago, however, Aristotle described the habits of a Grecian species differing much from those of the *Silurus glanis*, and Agassiz and Garman have recognized the old *Glanis* as a distinct species closely related to one of Asia (*S. asotus*); it is the *Silurus* (or *Parasilurus*) *Aristotelis*.

The old 'Introduction' purported (very mistakenly) to give the names of all genera supposed to be valid and diagnoses of very many of them. The new work merely gives the names of most of the genera of each family or only the 'principal genera.' None of the genera are diagnosed as many were in the introduction.

Typographical or authorial slips are not numerous. A few of them, however, might perplex the reader and consequently may be noticed here. The name *Anostomus*, properly used for a genus of Characinids (p. 576), also appears as a generic name under Mugilidæ (p. 640); *Agonostomus* is the actual name of the Mugiloid genus. Trichodontidæ is a family name of certain Perciformes (pp. 654, 633); the name appearing for fishes of the suborder Jugulares (p. 704) is merely a slip for Trichonotidæ (p. 706). In the statement that the family Lipogenyidæ 'has lessened the gap between the Lyomeri (Halosauridæ) and Heteromi (Notacanthidæ) of Gill,' Lyomeri (p. 622) is evidently a lapsus for Lyopomi. Lyomeri (p. 622) is properly the name of the group represented by Saccopharyngidæ. *Gnathacanthus* (p. 695) is a slip for *Gnathanacanthus*, the latter meaning exactly the opposite of the former. The Connecticut investigator of the origin of the lateral fins (James K. Thacher) is misnamed 'Thacker' (p. 245).

IX.

The differences between the new ichthyological school of Britain and that of America result chiefly from the different modes of approach to the subject. Dr. Boulenger had

long concentrated his attention chiefly on reptiles and amphibians, and the orders of those classes admitted by him are trenchantly separated by well-marked osteological characters. When he entered the ichthyological field he found that orders generally recognized in that class had not the same morphological value as the reptilian ones, and naturally groped around till he conceived he found a corresponding one in the group generally ranked as a subclass—Teleostei. The American naturalists took the orders as they found them left by their predecessors in the field, but a little examination and comparison showed that differences manifest within each of the large orders were of even greater morphological value than those used to differentiate the old orders. Some of those orders were consequently much contracted, as the Malacopterygii, Apodes, Anacanthini, Acanthopterygii and Plectognathi, and types ejected therefrom were set apart as of equal value, such as the Nematognathi, Plectospondyli, Symbranchii, Heteromi, Opisthomi, Pediculati and others. While these may not compare with the reptilian orders, they do with the mammalian and avian. One who has derived his knowledge of the orders of mammals and birds from a comparative examination of their skeletal features, and not from definitions in books alone, must admit that the average orders of mammals are not of greater morphological importance than the orders or 'suborders' of fishes, and that most of the orders of birds are of much less value. Likewise are the most contracted families of fishes of greater morphological value than most of those of birds—especially the Oscine birds—and of as great importance as the majority of those of mammals. A desire to establish for the fishes groups comparable with those adopted by the numerous students of birds and mammals has led American students to the narrow limitations of groups manifest in their works. The contrary method isolates ichthyology and gives a false or distorted idea of the significance of the terms order, family and genus. An expression of hope may be pardoned, therefore, that inasmuch as a long established standard for comparison has been adopted by many ichthyologists, others may in time recognize the propriety of accepting such a standard themselves.

The consideration of other differences must be left to other times and other places. Meanwhile we may congratulate European naturalists that the incubus which has long depressed ichthyology in the old world has been, to some extent at least, lifted, and that investigation may now be so directed that it will be profitable to systematic development. It was a bad and unscientific method that has paralyzed science in Europe for these many years, and let us hope that the new work may force it far into the background, if not wholly eradicate it. Let it be distinctly understood that the only sound foundation for scientific ichthyology is a profound *comparative* anatomy, and especially osteology of all the genera. This truth has long been recognized in the United States by some investigators, but it has not yet been appreciated by our museum authorities and in that respect the investigators of the old world and especially of London will for the present have a great advantage over Americans. We may envy our European colaborers, but shall be glad, nevertheless, to admire and avail ourselves of their superior advantages. We shall be grateful, also, for the new light which the coauthors of the 'Cambridge Natural History,' and especially Dr. Boulenger, have thrown and will continue to throw on mooted questions of morphology and classification. We thank them now.

THEO. GILL.

SCIENTIFIC JOURNALS AND ARTICLES.

THE March number of the *Botanical Gazette* contains the following papers: John M. Coulter and W. J. G. Land give an account of the gametophytes and embryo of *Torreya taxifolia*, a species localized in eastern Florida, and closely related to *Taxus*. The type seems to be specialized rather than primitive, with a solitary archegonium, remarkably early fertilization, and no 'open cells' in the proembryo. The peculiar 'rumination' of endosperm proves to be due to the irregular encroachment of endosperm upon perisperm. Pehr Olsson-Seffer discusses the principles of phy-

togeographic nomenclature, urging the gradual evolution of terminology rather than its rigid prescription. Harry N. Whitford continues his discussion of the forest of Flathead valley, Montana. J. C. Arthur suggests a set of simple terms for the spore structures in the Uredinales, whose confused terminology is at present extremely perplexing.

The contents of the *Journal of Comparative Neurology* for March are as follows:

Irving Hardesty: 'Observations on the Spinal Cord of the Emu and its Segmentation.' With four figures.

S. J. Holmes: 'The Selection of Random Movements as a Factor in Phototaxis.'

Walter C. Jones, M.D.: 'Notes on the Development of the Sympathetic Nervous System in the Common Toad.' With twelve figures.

Editorial:—Concerning the Genetic Relations of Types of Action. The Basis for Taxis and Certain Other Terms in the Behavior of Infusoria. The Problem of Instinct.

Isador H. Coriat: 'A Review of Some Recent Literature on the Chemistry of the Central Nervous System.'

SOCIETIES AND ACADEMIES.

THE GEOLOGICAL SOCIETY OF WASHINGTON.

The 165th meeting was held March 8, with President Merrill in the chair. The regular program included:

Genesis of Ore Deposits at Bingham, Utah: J. M. Boutwell.

Three types of ore were described: (1) lead-silver ore in lodes, (2) auriferous copper ore disseminated in monzonite and (3) bedded pyritous copper ore in marbleized limestone. The lode ores are believed to have been transported by heated solutions which ascended from a deep-lying magma along northeast-southwest fissures, and to have been deposited mainly by filling, partially by replacement. The disseminated ore was shown to have been formed at a period subsequent to the date of intrusion, partly from the original constituents of the monzonite and partly from copper, gold and other elements introduced by highly heated solutions or vapors. The immense lenticular beds of pyritous copper ore were formed by molecular replacement of contact metamorphosed limestone, partly from elements emitted by the intrusives contemporaneous with their intrusion and partly by subsequent additions from deep-lying portions of the magma after the superficial portions had at least partially solidified. Since these two periods of ore deposition the primary sulphides have been enriched by superficial alteration to oxides, carbonates, sulphates and secondary black sulphides. The complete report on this district which has been prepared by Mr. S. F. Emmons, Mr. A. Keith and Mr. J. M. Boutwell, is now in press and will appear shortly as Professional Paper No. 38.

The Subterranean Gases of Cripple Creek: Waldemar Lindgren.

A brief account was given of the gases which issue in some of the Cripple Creek mines and which as a rule were not encountered until a point below the zone of oxidation was reached. These gases interfere greatly at times with the work of those mines in which they appear and several deaths have been caused by them. It was found that the amount fluctuates according to the stand of the barometer. At low barometer it issues plentifully from the fissures and the porous rocks, and may fill the mine up to the collar. With high barometer the gas disappears and the change may take place very suddenly. The gas as a rule is heavy and accumulates in winzes and shafts. Its temperature appears to be somewhat higher than that normally prevalent in the mine. It was soon found that the amount of carbon dioxide, of which the gas was believed to consist, was entirely inadequate to produce the effects noticed, and analyses show that a great excess of nitrogen is present. A sample from the Conundrum mine was kindly analyzed by the department of chemistry at Cornell University, with the following result:

CO_2	10.22
O	5.7
N	84.1

Mr. W. H. Weed presented a paper on 'An Asphalt Lake near Tampico, Mexico.'

Geo. Otis Smith,
Secretary.

THE BIOLOGICAL SOCIETY OF WASHINGTON.

THE 397th regular meeting of the Biological Society of Washington was held February 11, 1905, with President Knowlton in the chair and sixty-eight persons present. Mr. Vernon Bailey exhibited a large mussel shell, about 13 x 8 cm. in size, from Trinity River in southeastern Texas. He noted that the species is a common food for raccoons. He stated that many pearls are gotten from the region mentioned, and that one had sold for sixty dollars. Dr. L. O. Howard called attention to the first authentic record from the Pacific coast of Mexico of the presence of the yellow fever mosquito (*Stygeomia fasciata*) in this region. The authenticity of the record rests in specimens sent to Dr. Howard by a physician.

The first regular paper of the evening was by Dr. Albert Mann on diatoms. The speaker introduced his subject by referring them to the Conjugatæ of the green algæ, and gave several reasons for this systematic position.

He then took up their distribution, stating that they are to be found in all latitudes and all waters, fresh, brackish or salt; that the tropical are the largest and most ornamental, but they are most prolific in individuals in arctic waters. Geologically he placed their first appearance in the Upper or Middle Cretaceous, stating that the claims of Castracane and others of their presence in the coal measures or even lower are inconclusive. Many of the great beds of diatom earth were referred to.

The box-like structure of these algæ was next illustrated by drawings and the elaborate ornamentation of the valves described. Outside of this silica case the organic pellicle, erroneously called a 'gelatinous' sheath, and inside the case the large symmetrical chloroplasts, the cytoplasm, nuclei, vacuoles and oil globules were explained by diagrams.

Under the physiology of the diatoms the speaker explained the normal processes of plant assimilation and stated that their dependence on sunlight precluded their being found living in subterranean waters or deep-sea beds; the limit of the latter he placed at 100 fathoms. Saprophytic diatoms, *Nitzschia putrida* and *Nitzschia paradoxa* were mentioned, the latter semi-saprophytic.

The multiplication of the diatoms by fission was illustrated by several diagrams and the consequent progressive reduction in size commented on. Sexual reproduction, by which the full dimensions were regained, was then described and illustrated, three variations in method being mentioned.

The mystery of diatom movement was discussed and the various theories explained.

Dr. Mann closed his paper by briefly enumerating the economic uses of the diatoms and by a short description of methods of collection. He then exhibited forty-one lantern slides prepared from the negatives of the Hon. A. A. Adee, assistant secretary of state.

The second paper was by Dr. Edgar A. Mearns, on the 'Animal Life of Mount Apo, of the Philippine Islands.' He said, as the result of a month's examination of the solfataric volcano Apo, the giant of the archipelago, its animal life became sufficiently familiar to admit of at least a partial comparison with that of the better explored mountains of Luzon and other islands of the Philippine group. The constituent fauna of Mount Apo partakes largely of peculiar elements, which isolate it not only from the lowlands of Mindanao, but from the highlands of other Philippine islands, and give it a faunal position comparable in importance to that of Monte Data in Luzon and Kina Balu in Borneo. Several of the genera and most of the species of mammals collected on the higher portion of Apo are new to science; and three genera and more than a score of species of birds have been recently added to the Philippine avifauna from Mount Apo. Dr. Stejneger has recently described two new species of frogs and one new Gecko from the speaker's Mount Apo collection.

THE 398th regular meeting of the society was held February 25, 1905, with President Knowlton in the chair and thirty-seven persons present. Under Notes, Dr. C. E. Waters referred to the paper of the preceding meeting by Dr. Mann, on diatoms, and to papers by Kramer and White, and asked if it were

probable, as suggested, that petroleum was formed from the oil found in the protoplasm of diatoms. This was answered in the negative by Dr. Mann. Dr. E. L. Greene asked if any of the biologists present knew of a Rafinesque other than the one who has contributed so fully to American science. No one replied, and Dr. Greene then stated that in a list of botanical authors, printed at Zurich, Switzerland, in the year 1772, occurs the name of P. I. Rafinesque. He is credited with the authorship of an essay on economic botany, purporting to have been published in 'Memoirs of the Society for Economics,' at Berne, the date of the volume being 1763.

The first regular paper of the evening was by Dr. E. L. Greene, on the 'Earliest Local Flora.' The speaker gave a sketch of the 'Flora Hercynia, a Catalog of the Plants of the Harz Mountains,' by Dr. Joahnes Thalius, published at Nordhausen, Germany, in 1588; a work in which many new plants are named and defined, besides several genera; among these *Alsinanthemum,* a type known since Linnæus by the name of *Trientalis;* also even that more than two centuries later indicated as if new under the name *Eleocharis* R. Br.

The second paper on the program was by Mr. David White, on 'Fossil Plants of the Group Cycadofilices.' This paper was profusely illustrated with lantern slides. Mr. White gave a synopsis of the Pteridospermeæ, describing the more important and interesting anatomical characters of the stems and petioles, and illustrating a number of foliar types and fruits more or less definitely correlated with the other parts of the plants. A number of genera, including *Eremopteris, Triphyllopteris, pseudopecopteris* and *Megalopteris,* were tentatively referred to the pteridosperms, which are to be regarded as comprising the most characteristic plant life of the Carboniferous. Attention was called to the antiquity (Middle Devonian) of one of the types, *Kalymma grandis,* and the consequent probable antiquity of the heterosporous Filices which must have antedated the Cycadofilices.

E. L. Morris,
Recording Secretary.

THE NEW YORK ACADEMY OF SCIENCES. SECTION OF ASTRONOMY, PHYSICS AND CHEMISTRY.

The regular meeting of the section was held at the American Museum of Natural History on Monday evening, March 20. The program consisted of the following papers:

The Sixth Satellite of Jupiter: S. A. Mitchell.

Dr. Mitchell gave an interesting account of the recent discovery of a sixth, and also a seventh, satellite of Jupiter by Professor C. D. Perrine at the Lick Observatory, and described the details of the photographic method by which these satellites were discovered last December and January.

Dr. Mitchell also spoke of the discoveries of satellites of the other planets, including the ninth satellite of Saturn which was found by Professor W. H. Pickering in August, 1899.

A Pocket Form of the New Piezic Barometer: Ernest R. von Nardroff.

The Piezic barometer measures the atmospheric pressure by measuring the *elasticity* of a portion of air. In the small pocket form of the instrument exhibited, a piece of heavy barometer tubing of 3 mm. bore and about 12 cm. long was provided at its lower end with a pear-shaped bulb, having an internal volume equivalent to about 70 cm. length of the tube. At its upper end the tube was provided with a second small bulb containing about 1 c.c. of mercury. Entering into the tube from above was a short tube having at its lower end a capillary opening. Through this tube the mercury was introduced.

In using the instrument all the mercury is brought into the upper bulb by inverting. The instrument is then turned into the erect position, when the mercury enters the main tube a few centimeters, the exact distance depending upon the atmospheric pressure. The less the pressure and hence the less the elasticity of the air, the more the mercury will enter. The mercury stands in the upper portion of the tube and partly in the upper bulb without any tendency to run down the sides of the tube. A scale on the main tube drawn *by comparison* with a standard barometer indicates the pressure.

To understand the theory of the instrument assume the lower bulb replaced by a continuation of the barometer tubing of equal volume. Let b stand for the standard barometer height, m for the length of the thread of mercury entering the tube, and a for the length of the column of compressed air. Then from Boyle's law ($pv = p'v'$) we have

$$b(a+m) = (b+m)a,$$
$$b = a,$$

and hence

$$\Delta b = \Delta a.$$

That is, the divisions of the scale on the Piezic barometer are of the same size as those on the ordinary barometer. However, in practise the upper bulb always contains some mercury after the air is entrapped. The general effect of this is to make $\Delta a < \Delta b$.

The Exhibit of the U. S. Geological Survey Radium Collection shown at the St. Louis Exposition: G. F. Kunz.

Mr. Kunz described the object of and the success of the exhibit, stating that many of the most eminent investigators, including Sir William Crookes and Professor Rutherford, had sent their original material. The collection was shown in an upper hall of the museum. There was also exhibited the Kunz 1,081-pound mass of Cañon Diablo meteoric iron, the largest mass known of this meteoric iron. Mr. Kunz stated that Professor Henri Moissan, of Paris, had discovered in dissolving 183 pounds of this material (Cañon Diablo meteorite), not only crystalline diamonds, but the crystalline substance, carbon silicide, never before discovered as a natural product, but extensively manufactured and used in the arts under the name of carborundum. In view of the many eminent discoveries of Professor Moissan in the field of chemistry and electrometallurgy, as well as in the study of meteorites and of diamond formation, Mr. Kunz suggested that this mineral be named *moissanite* in his honor.

Experimental Research concerning Indirect and Secondary Skiagraphic Rays: L. G. Cole.

The immediate discharge from an X-ray tube consists of two distinct classes of so-called rays—direct and indirect. The direct rays have their inception at the focal point of the anode and radiate in direct lines and are not reflected, but deflected, and do not set up secondary rays, but are absorbed by the tissue of the body in proportion to the amount of solids contained therein.

The indirect rays radiate from the walls of the tube, are projected at various angles, and cause secondary rays in objects with which they come in contact, especially the soft tissue, and give great penetration. The effect attained depends on the amount of current, frequency of interruption and molecular action of glass.

Dr. Cole then described the life history of a tube, stating that definite changes occur in a tube when used, including a crisis, and explained the difference between the action of new and seasoned tubes and the difficulty of exciting very old tubes.

He also gave his opinion of the cause of the purple color of the glass of a tube and suggested that there is a molecular rearrangement of glass similar to that occurring in steel when magnetized. In a new tube the direct rays amount to 30 to 40 per cent., while in some seasoned tubes as much as 75 to 90 per cent. Furthermore, the indirect rays project themselves behind the bones, causing a lack of definition of bones and obliteration of detail of soft parts, while direct rays give detail in soft parts, showing even the blood in the veins.

Dr. Cole, who is skiagrapher at Roosevelt Hospital, will publish his paper in full in the *Archives of the Röntgen Ray Magazine.*

C. C. Trowbridge,
Secretary.

THE SCIENCE CLUB OF THE UNIVERSITY OF WISCONSIN.

The sixth meeting of the club for the year 1904–5 was held on Tuesday, March 21, at 7:30 P.M. in the physical lecture room, Science Hall.

The first paper on the program, by Dr. R. Fischer, treated the subject 'Food Adulterations and their Detection.' The various methods of adulteration of human foods met with this state, and the methods of their

detection, were explained by the speaker, and a large array of choice specimens of adulterated goods were shown which furnished strong testimony to the cupidity of some manufacturers of food articles and, in many cases, to their lack of regard for human health.

The second paper of the evening, by Dr. C. A. Fuller, was on the subject, 'The Dissemination of Typhoid Fever by Oysters.' Outbreaks of this disease have occasionally been traced to infected oysters. Bacteriological examinations of these shell-fish usually demonstrate the presence of bacterium coli. The survey of 3,000 acres of oyster ground in Rhode Island waters showed that both water and oysters from sections within six miles of the outlet of the capital city sewer contained sewage bacteria, while samples taken at a greater distance from the source of contamination were not infected; similar conditions were observed to prevail in a number of the oyster beds on the east coast of the United States.

F. W. Woll,
Secretary.

MEETING OF EXPERIMENTAL PSYCHOLOGISTS AT CLARK UNIVERSITY.

The second yearly meeting of teachers and students of experimental psychology was held in the Clark University laboratory, at the invitation of Professor E. C. Sanford, on Friday and Saturday, March 31 and April 1. At the first session, on Friday afternoon, papers were read by Mr. L. M. Terman, on 'Tests of Bright and Dull Boys'; by Mr. A. L. Gesell, on 'Handwriting and Scholarship'; and by Mr. W. F. Book, on the 'Learning of Typewriting.' The visitors then inspected the laboratory, under the guidance of Professor Sanford, who demonstrated, among other instruments, two devices for the determination of the temporal limen of disparate sense impressions, a rotating-prism color mixer, and an apparatus for investigating the sensible discrimination of purple. Professor A. H. Pierce, of Smith College, next described three researches now in progress in his laboratory; and a paper by Professor Max Meyer ('Auditory Sensations in an Elementary Laboratory Course') was read by Mr. H. C. Stevens, of Cornell University. A discussion followed, in which Professors Pierce, Sanford and Titchener took part. In the evening the visiting psychologists were entertained at dinner by Professor Sanford.

The session of Saturday morning was opened by President Hall, with a paper on 'Some Tendencies and Dangers of Experimental Psychology.' The paper was discussed by Dr. Hylan and Professor Titchener. Adjournment was then made to the physical laboratory, where Professor A. G. Webster demonstrated his apparatus for the measurement of the objective intensity of sound. The last hour of the morning was spent in further inspection of the psychological laboratory, after which the guests enjoyed the opportunity of meeting the Clark University students at a luncheon given by President Hall.

At the afternoon session Professor Bentley, of Cornell University, read a paper on the 'Analysis of Tones,' and afterwards demonstrated his method and certain of his results. The meeting was fittingly concluded by an inspection of the new library building, arranged by Mr. L. N. Wilson. All psychologists know the treasures of the Clark University library, and the willing courtesy of their custodian; and all present on this occasion were delighted with the disposition and conveniences of the library.

It was decided to accept Professor Judd's invitation to hold the meeting of 1906 in the Yale University laboratory.

DISCUSSION AND CORRESPONDENCE.

ALTERNATION OF GENERATIONS IN ANIMALS.

In the February number of the *Botanical Gazette,* Dr. C. J. Chamberlain writes, "After hesitating for several years I have decided to publish my belief that animals exhibit an alternation of generations comparable with the alternation so well known in plants. In short, the theory is this: the egg with the three polar bodies constitutes a generation comparable with the female gametophyte in plants; similarly, the primary spermatocyte with the four spermatozoa constitutes a generation comparable with the male gametophyte

in plants. All other cells of the animal constitute a generation comparable with the sporophytic generation in plants, the fertilized egg being the first cell of this series."

In the diagrams employed in the exposition of his theory he indicates that the animal egg by itself and each spermatozoid is comparable to a plant gametophyte. His statements are not consistent, not in accordance with the facts or even with his figures, and it appears that just where he wishes to draw the homology is not quite clear in his own mind.

Our knowledge of animal phylogeny affords no evidence that the gametes, with their reduced number of chromosomes, are vestigial individuals which at one time in their history lived independent of or apart from the animal body. They do not constitute and there is no evidence that they ever have constituted, a *generation* in the life-history of any animal organism. If amphimixis occurs in the life-history of an organism, a reducing division must also occur. The mechanism of reduction seems, in general, to be bound up in two successive mitoses. That the cytological processes of reduction in plants and animals closely approximate a common plan does, by no means, justify the conclusion that the products are of the same morphological value in the life-cycles of each.

Chamberlain says: "To me the comparison seems so obvious that I can explain the previous absence of a theory of alternation of generations in animals only by the fact that the gamete-bearing generation is extremely reduced and is not approached by any gradual series as in plants. * * * I do not claim any acquaintance with zoological literature further than a reading of the latest edition of Wilson's 'The Cell in Development and Inheritance.' Were there any theories as to alternation of generations in animals, doubtless they would have been thoroughly discussed in that book."

That zoologists recognize an alternation of generations in the Hydrozoa and Scyphozoa is a common statement of their text-books. That a theory of antithetic alternation of generations in the life-histories of animals has been propounded by certain zoologists, Beard and Murray,* does not require a knowledge of zoological literature to determine, for it occupies a conspicuous place in a prominent botanical journal as well.

In the course of their discussion Beard and Murray write: "When one seeks in the higher animals for an equivalent of the alternation of generations in plants in the light of recent work on the reducing division of spore-formation, such a morphological mark would only be found in the maturation of the egg and in spermatogenesis. If the process were here a spore-formation, the whole metazoan body, in which it took place, would represent the asexual generation, and any apparent alternation of generations in the life-cycle would be homologous in character, not antithetic."

In speaking of the reduction of chromosomes in the oogenesis of *Fucus,* Farmer and Williams† call attention to this same analogy in the following sentences: "Thus *Fucus,* in this respect, approximates more closely to the type of animal oogenesis than to that which obtains in those higher plants in which the details of chromosome reduction have been followed out. Regarded from the standpoint of the number of its chromosomes, the *Fucus*-plant resembles the *sporophyte* of the higher plants, whilst the gametophyte of the latter, with its reduced number of chromosomes, finds its analogue merely in the maturing sexual cells of *Fucus.*"

Harold L. Lyon.
University of Minnesota.

SCIENCE AND THE NEWSPAPERS.

To the Editor of Science: Recently three Chicago newspapers (the *Record-Herald,* the *Tribune* and the *Chronicle*) published, without our knowledge or consent, an alleged account of experiments communicated by us to a meeting of physiologists. It is needless to state that this account was quite misleading. We at once sent the enclosed letter to the papers in question. Only one of them (the *Record-Herald*) pursued the fair and manly course of publishing it. The *Tribune* did not deign even to acknowledge receipt of our let-

* *Anat. Anzeiger,* **11**: 234–255. *Ann. of Botany,* **9**: 441–468.

† *Ann. of Botany,* **10**: 479–487.

ter. The *Chronicle* refused to print it, but offered to correct any misstatements in its article, an illusory offer in relation tp such a tissue of inaccuracies, and one which we had no desire to accept.

We think it right that the scientific professions should know the attitude which the conductors of some newspapers consider themselves justified in adopting towards scientific workers, and we wish to record in your columns, once for all, that protest which they have not permitted us to make in theirs.

G. N. STEWART,
C. C. GUTHRIE.

CHICAGO, APRIL 3, 1905.

Sir:—In yesterday's issue of your paper there occurs a garbled and misleading account of certain experiments communicated by us to a meeting of physiologists of the central states. We are entirely opposed to the discussion of such matters in the lay press. If any reporter was present at our meeting he certainly was there without invitation or permission. We do not know from what source this remarkable piece of copy reached your office. But we can not think the writer has fully considered how injurious such notices may be to the reputation of scientific investigators; and while we entertain the greatest respect for your paper in its proper sphere, we must beg of you in the future to do us the honor of leaving us and our work alone. We trust that you will give this letter the same publicity as the paragraph to which we object.

We remain, yours truly,
(Signed) G. N. STEWART,
C. C. GUTHRIE.

A MODEST STUDENT OF ANIMAL PSYCHOLOGY.

IN the preface to 'The Watchers of the Trails' its author, C. G. D. Roberts, writes:

The psychological processes of the animals are so simple, so obvious, in comparison with those of man, their actions flow so directly from their springs of impulse, that it is, as a rule, an easy matter to infer the motives which are at any one moment impelling them. In my desire to avoid alike the melodramatic, the visionary and the sentimental, I have studied to keep well within the limits of safe inference. Where I may have seemed to state too confidently the motives underlying the special action of this or that animal, it will usually be found that the action itself is very fully presented; and it will, I think, be further found that the motive which I have here assumed affords the most reasonable, if not the only reasonable, explanation of that action.

On page 221 of the same book the author writes:

As the raccoons crept along behind the woodshed they smelt traces of a sickly pungent odour, and knew that other marauders had been on the ground not very long before. This made them bolder in their enterprise, for they knew that such depredations as they might commit would be laid to the account of the skunks, and, therefore not likely to draw down vengeance upon the [raccoon's] den in the sycamore.

MAYNARD M. METCALF.

THE WOMAN'S COLLEGE OF BALTIMORE,
March 19, 1905.

A NEW FORM OF STEREOSCOPE.

TO THE EDITOR OF SCIENCE: I read with interest Professor Whitman's account of his new form of stereoscope in your issue of April 7. I have described the same type of instrument in SCIENCE, Vol. VII., p. 619. I was led to the invention thereof by the instrument called the perspectoscope which mistakenly attempted to get a stereoscopic effect from a single photograph, but in doing so used the convenient device of placing the eyes at right angles to the picture. Using this principle, I made an apparatus with pivoting mirrors which enabled me to throw one of a pair of stereoscopic images into the one eye, and the other into the other, just as Professor Whitman has independently done. I have used this both in combination with weak lenses and without them. I have had such an apparatus in my laboratory for about seven years.

The main advantage of the instrument (its defects are well defined by Professor Whitman) for the psychological student is that it offers a simple means of reversing the perspective without changing the card, throwing the image of the right-hand picture into the right or left eye and correspondingly for the left eye, thus producing a stereoscopic or a pseudoscopic effect; indeed, an intermediate position in which the same view is thrown into each eye is also possible and thus gives the entire range of combinations. The Chicago

Laboratory Supply Company is now making some of these instruments to serve as reversible stereoscopes. In their manufacture the difficulties of projection of the two images to differently situated planes have been encountered, and have been met only by reducing this to a minimum and counting upon the fortunate property of the eyes to ignore or, indeed, to make terms of peace with this discrepancy. I find that this is easy, when an ordinary photograph with no sharp gradations of light and shade is used; but with diagrams the non-correspondence of top and bottom is moderately disturbing. I have not hesitated for purposes of convenience to combine lenses with this reflecting stereoscope; but I shall profit by Professor Whitman's suggestion to see how far the increased proximity of the eyes to the mirrors, which he recommends, will obtain certain of the advantages which I tried to secure by weak lenses. This last variation is a detail of construction in which Professor Whitman's device differs from mine.

Joseph Jastrow.

University of Wisconsin,
April 8, 1905.

SPECIAL ARTICLES.

A REVISION OF THE COCCACEÆ.*

The classification of the bacteria presents peculiar difficulties for several reasons. Morphological distinctions are so slight that physiological characters must necessarily be invoked in order to separate and classify the various organisms, and these physiological characters are often variable. Pathogenicity may be taken as a type of those powers of the organism which are easily and profoundly modified by external conditions. On the other hand, there are numerous characters which appear to be extremely constant. Such minute differences as occur in the resistance of different races to unfavorable conditions often remain unchanged through long periods of cultivation. In using these constant characters for classification we are met by another difficulty. Though constant, the differences are very minute, and in studying a number of organisms a perfect gradation is often found between the widest extremes. This is exactly what should be expected from organisms which reproduce only by asexual methods since it is the fusion of independent cells which swamps minor differences producing the uniformity of species among higher plants. With asexual reproduction every minute variation which is inheritable must persist unchanged until some other chance variation occurs. Each such variation means a new and different type of bacterium.

The immense number of generations which may succeed each other in a short space of time makes boundary lines as shifting as they would become among the higher plants if a dozen geological epochs were considered all at once.

Since with unicellular organisms acquired characters may probably be inherited in a higher degree than with other forms, existing races of bacteria will be markedly influenced by the selective effect of environmental conditions, and must bear the impress of their recent history.

There are, therefore, no species among the bacteria in quite the sense in which we ordinarily use the word,—as indicating a group of individuals bound together by a number of constant characters and easily identified by mutual fertility. From one point of view each distinct race might be considered a species; but to apply a name for every grade of difference in each varying character would be impracticable; and such names could have no true specific value. The best solution of the difficulty is the establishment of certain types around which the individual organisms may be more or less closely grouped; but it must be clearly recognized that the groups thus formed are defined by relation to the type at their center and are not sharply marked off at their extremities from the other groups adjacent.

It is impossible to make a natural classification of the bacteria, which shall be a true expression of phylogeny by considering a single character at a time,—for example, by dividing a group dichotomously, first according to mor-

* Preliminary communication. From the Biological laboratories of the Massachusetts Institute of Technology.

phology, then according to liquefaction, etc. Larger groups at least should manifestly be indicated by the collocation of several characters, the association of any two of which markedly strengthens their significance. By applying this principle, five fairly well-marked genera of coccaceæ may be distinguished. Four of these, *Sarcina, Micrococcus, Streptococcus* and *Ascococcus* date back to the early days of bacteriology, although the latter term has fallen into disuse. The mere property of zoöglea formation should not be considered of generic importance, but the few peculiar species which are capable of growing under purely saprophytic conditions and producing large gelatinous masses, are so far marked off from other cocci as to warrant, in our judgment, the retention of Cohn's genus. The genera *Micrococcus, Streptococcus* and *Sarcina* are retained, since in them morphological differences appear to be correlated with differences in biochemical characters or habitat and we have considerably enlarged the definitions of these genera to include physiological and ecological factors. With regard to the genus *Diplococcus* suggested by Weichselbaum for the parasite of pneumonia it should be remembered that any coccus may at times occur in pairs. Yet those organisms which are strictly parasitic and which normally occur only in aggregations of two cells appear to mark a valid group. The morphological character of a genus must never be too rigidly interpreted. It refers to the typical and most commonly characteristic growth forms; and other groupings may at times occur. Therefore, we recognize five genera, based in each case on a more or less constant association of several independent characteristics. The old genera *Merispomedia* and *Staphylococcus* are merely synonyms of Cohn's *Micrococcus,* and *Ascococcus* antedates *Leuconostoc.* Fischer's characterization of *Pediococcus* by regular division into two sections at right angles to each other, rests upon a variable and thoroughly artificial character and probably includes some species of *Micrococcus* and some imperfectly studied species of *Sarcina.*

With regard to the genera, *Planosarcina* and *Planococcus,* founded upon the single characteristic of the possession of flagella there may be more uncertainty. The slow revolution and steady translation observed by Ali-Cohen and Migula as associated with flagella, is certainly a phenomenon distinct from the irregular vibratory and rotary movements noted by other observers, but the resemblance between motile and non-motile forms is so close in all other characters that we can not consider this single property to be of generic importance.

The five genera above mentioned have been discussed first because their characteristics are already somewhat familiar; but in logical order the larger subdivisions should have been previously considered.

The family Coccaceæ, although defined only by the spherical form of the individual, is a thoroughly satisfactory natural group, its members being also marked off in certain physiological characters from individuals of other groups. The family appears to be divisible into two subfamilies. The first, for which we suggest the name Paracoccaceæ (paratrophic cocci), includes *Diplococcus* and *Streptococcus,* parasitic forms which do not develop abundant growth on artificial media and which thrive better under anaerobic than under aerobic conditions, and appear in small cell aggregates of pairs or chains. The second subfamily, the Metacoccaceæ (metatrophic cocci), includes *Micrococcus, Sarcina* and *Ascococcus,* saprophytic or semi-saprophytic types which are aerobic and form abundant surface growths of large cell groups.

The species of the coccaceæ are considerably more obscure. We have reviewed the descriptions of 445 supposedly distinct species given by Cohn, Migula, Flügge, Chester, Sternberg, Lehmann and Neumann, Engler and Prantl, Rabenhorst, Frankland and Woodhead and find a wonderful amount of duplication. Our observations have convinced us that minute differences in morphology, as for example, the distinction between large and small cells or long and short chains, are not sufficiently constant for the erection of species. Again, slight differences in the appearance of colonies on gelatin, which form a large number of German species, vary

so markedly, according to the composition of the medium and conditions of incubation, that they may be disregarded. The turbidity and sediment in broth varies with the age of the culture: what is first turbidity later settles to form sediment and those constant differences which do exist appear to be connected with the size of the cell aggregates. Organisms growing in large groups like most of the Sarcinæ produce heavy sediment and often colony-like groups on the walls of the tube, while those in which the cells readily separate exhibit a more diffuse turbidity. The growths on potato and Nährstoff agar are correlated with the general vigor of a particular race and vary markedly. Temperature relations are similarly inconstant and what marked differences exist are correlated with other characters to which we have given weight; for example, the Streptococci, as a rule, thrive best at the body temperature, while the Sarcinæ and many Micrococci grow better or as well at 20°.

There remain then for the establishment of species the relation of the organism to gelatin, its action upon sugars, its pigment production and its power to form nitrites and indol. In regard to all these points much more thorough study is needed. In particular, almost no data exist with regard to indol and nitrite production. By using the first three characters with one or two others which are of importance in special cases we have made a tentative division of the 445 described forms under thirty-one types. A careful comparison of the published descriptions furnished no evidence for more true species and 85 cultures, isolated from various sources, and obtained from the principal American laboratories, which we have studied in considerable detail, fall naturally under some one of the types established. *S. erysipelatos* includes 20 of these cultures, *M. aureus* 11, *M. orbicularis* 8, *M. ochraceus* 5, *M. ureæ* 3, *M. canescens* 5, *M. candicans* and *ventriculi* 2 each, *M. luteus, M. cinnabareus, M. æthebius, S. subflava, S. incarnata* and *S. aurantiaca* 1 each. It is very probable that our further investigation will warrant the division of some of these types but we present the thirty-one species below tentatively and subject to later revision. It must be understood as noted above that in all cases the names mark only types, numerous intermediate races existing between.

FAMILY COCCACEÆ.

Vegetative cells spherical.

Subfamily 1. PARACOCCACEÆ (*new subfamily*).

Parasites (thriving only, or best, on, or in the animal body). Thrive well under anaerobic conditions. Many forms fail to grow on artificial media, none produce abundant surface growths. Planes of fission generally parallel, producing pairs, or short or long chains.

Genus 1. *Diplococcus* (Weichselbaum).

Strict parasites. Not growing or growing very poorly, on artificial media. Cells normally in pairs, surrounded by a capsule.

Under *Diplococcus* are three species, *D. pneumoniæ*, Weich., *D. Weichselbaumii*, Trev. and *D. gonorrhϾ*, Neisser—distinguished by the tissue of the host affected, and by the peculiar morphology and staining reactions of the latter species.

Genus 2. *Streptococcus* (Billroth).

Parasites (see above). Cells normally in short or long chains (under unfavorable cultural conditions, sometimes in pairs and small groups—never in large groups or packets). On agar streak effused translucent growth often with isolated colonies. In stab culture, little surface growth. Sugars fermented with formation of acid.

Under *Streptococcus* we find the vast majority of organisms indistinguishable from *S. erysipelatos*, Fehleisen. Two varieties may perhaps be recognized, var. *involutus* Kurth; and var. *tenuis* (new variety), which fails to coagulate milk. Representatives of another species, *S. enteritidis* Escherich, which liquefies gelatin are occasionally found.

Subfamily 2. METACOCCACEÆ (*new subfamily*).

Facultative parasites or saprophytes. Thrive best under aerobic conditions. Grow well on artificial media, producing abundant surface growths. Planes of fission often at right angles; cells aggregated in groups, packets or zoöglea masses.

Genus 3. *Micrococcus* (Hallier) Cohn.

Facultative parasites or saprophytes. Cells in plates or irregular masses (never in long chains or packets). Acid production variable.

Under this genus, thirteen species may be distinguished, by the three properties of liquefaction, acid production and chromogenesis, their characters being indicated in tabular form below.

GELATIN LIQUEFIED.

	ACID.	NON-ACID.
Yellow	*M. aureus* (Ros.) Mig.	*M. orbicularis* (Ravenel)
White	*M. pyogenes* (Ros.) Mig.	*M. rhenanus* Mig.
Red	*M. roseus* Flügge.	*M. fulvus* Cohn.

GELATIN NOT LIQUEFIED.

	ACID.	NON-ACID.
Yellow	*M. luteus* (Schroter) Cohn.	*M. ochraceus* Rosenthal.
White	*M. candicans* Flügge.	*M. canescens* Mig.
Red		*M. cinnabareus* Flügge.

M. ureæ Cohn. Ammoniacal fermentation of urine produced. Gelatin not liquefied.

M. æthebius Trevisan. Ammoniacal fermentation of urine produced. Gelatin liquefied.

Genus 4. *Sarcina* (Goodsir).

Saprophytes or facultative parasites. Division under favorable conditions, in three planes, producing regular packets. Sugars as a rule not fermented.

Under *Sarcina* are eleven species, eight of which are grouped as follows:

	Gelatin Liquefied.	Gelatin not Liquefied.
Yellow	*S. subflava* Ravenel.	*S. ventriculi* Goodsir.
White	*S. candida* Lindner.	*S. pulmonum* Virchow.
Red	*S. rosacea* Lindner.	*S. incarnata* Gruber.
Brown	*S. cervina* Stub.	*S. fusca* Gruber.

In addition three somewhat aberrant species must be recognized—*S. aurantiaca* Flügge, a yellow liquefying chromogen, which unlike the other members of the group, has the power to coagulate milk, with *S. agilis* (Ali-Cohen) Mig. and *S. tetragenus* (Mendoza), Mig., respectively red, and yellowish-white, motile forms. From study of the literature and a few cultures of supposedly motile forms we are inclined to believe that all the truly motile cocci may be classed under these two heads.

Genus 5. *Ascococcus* (Cohn).

Generally saprophytic. Cells imbedded in large irregularly lobed masses of zoögloea, in presence of carbohydrates. Acid usually formed.

Two species are distinguished, *A. mesenteroides* Cienkowski, a non-liquefying form, and *A. mucilaginosus* Migula, a liquefier.

The characters of the species tentatively defined above are still somewhat artificial and may be subject to revision and modification when our studies are complete. It is probable that the liquefaction of gelatin can not bear any very direct relation to phylogeny, since in every genus except *Diplococcus*, and in each subdivision of a genus, a liquefying and a non-liquefying form occur parallel to each other.

Synonymy will be discussed in our full communication later; but we have strictly followed the rules of priority as recognized in other fields of systematic biology.

In reviewing our genera a serial arrangement is at once apparent. *Diplococcus* is strictly parasitic, and commonly produces only aggregates of two cells. *Streptococcus*, also normally parasitic, thrives better, though still not luxuriantly, on artificial media and its typical growth-form is a chain. *Micrococcus* includes both pathogenic and non-pathogenic forms but all grow abundantly on gelatin and agar, in rather large irregular cell aggregates, while some produce acid and some alkalies in milk. *Sarcina* shows further development in the same direction, its growth form being larger and produced by three planes of division, its saprophytic habit being more marked, (no truly pathogenic forms known to exist), with the power of acid production generally wanting. *Ascococcus*, in spite of its slight acid production and chain formation, appears on the whole to form the extreme of this series, since its entirely saprophytic existence and large vegetative growth-forms are far removed from the pathogenic micrococci. The genera above defined seem to mark the important transition stages beginning with such strict parasites as *D. Weichselbaumii* and ranging through the intermediate forms of Streptococci, Micrococci and Sarcinæ to the saprophytic *Ascococcus mesenteroides* at another extreme. We believe that these genera have true phylogenetic significance and represent real groups of organisms having natural affinities.

C.-E. A. WINSLOW,
ANNE F. ROGERS.

A CONNECTION BY PRECISE LEVELING BETWEEN THE ATLANTIC AND PACIFIC OCEANS.

On October 4, 1904, a Coast and Geodetic Survey party, running eastward from Seattle, Wash., met a similar party, running westward, at Hunts Junction, in the southeastern part of Washington. The party running from east to west had started in the beginning of the season from a bench mark of which the elevation had been fixed by a long line run during several seasons and extending westward from the precise level net, composed of many circuits, which covers the eastern half of the United States. As far west as Norfolk, Nebr., the elevations in this net had been checked by completed circuits of precise leveling of the highest grade of accuracy. The joining of the two lines at Hunts Junction completed the first connection by precise leveling between the Atlantic and the Pacific.

The discrepancy developed at the junction was .615 ft. (= 187.5 millimeters), the Pacific being apparently higher than the Gulf of Mexico and Atlantic.

The old question at once arises: is the Pacific at a different elevation from the Atlantic? The discrepancy of .615 ft. must be due either to errors in the tidal observations which furnished the connection with mean sea level, or errors in the leveling, or to a real difference in the elevation of the mean sea surface at the points at which the tidal observations were made.

The three principal connections with sea level concerned are at Sandy Hook, near New York City, at Biloxi, Miss., and at Seattle, Wash. Six years of tidal observations were taken at Sandy Hook and five years at each of the other points. The range in the six annual means at Sandy Hook was .322 ft., and of the five annual means at the stations at Biloxi and Seattle was, respectively, .100 and .204 ft. These ranges are not sufficient to account for the discrepancy of .615 ft.

The shortest line of leveling of the highest grade of accuracy from Seattle to Sandy Hook is 4,600 miles (7,400 kilometers); to Biloxi, 3,500 miles (5,700 kilometers); and to Norfolk, the point at which the line to the westward leaves the thoroughly checked portion of the precise level net, is 2,000 miles (3,300 kilometers).

If it is assumed that the discrepancy (.615 ft.) is simply an accumulated error in leveling and that the rate of accumulation is uniform between Seattle and Biloxi, it is at the rate of one foot in 5,700 miles (.033 millimeters per kilometer). Even if it is assumed that the accumulation all occurred between Seattle and Norfolk, it is at the rate of one foot in 3,300 miles (.057 millimeters per kilometer). This is an extremely small error of leveling.

Another test to determine whether the discrepancy is a possible error of the leveling may be applied. The probable error of the elevations at Seattle, as carried westward from the Gulf and Atlantic, the computation being based upon the discrepancy developed in the circuits in the eastern part of the United States, was $\pm$ 76 millimeters. The actual discrepancy is two and one half times this. According to the doctrine of chances, such a discrepancy, two and one half times the probable error, should occur about once in ten times.

Therefore, it is not safe to make the statement that the Pacific is higher than the Gulf and Atlantic; the extremely small discrepancy being well within the possible limits of error of the precise leveling alone, even though it be assumed that the leveling in question is of as high a grade of accuracy as any yet done anywhere in the world.

One is apt to associate observations of such extreme accuracy as this precise leveling with slow progress and high cost. It is interesting, therefore, to note that three thousand miles out of the thirty-five hundred between Seattle and Biloxi have been leveled since the beginning of the field season of 1899, and that the average rate of progress, during the period 1900–1904 (a total of 3,900 miles), with the new type of precise level now in use in the Coast and Geodetic Survey, was 64 miles of completed line per month, for each observer, and that the average cost, per completed mile, was $10, including salaries, transportation and bench marks. Each completed mile was leveled over at least twice, and, in some cases, four or more times. This rate of progress is

comparable with that ordinarily secured in wye leveling, which is of a much lower order of accuracy.

JOHN F. HAYFORD.

BOTANICAL NOTES.

RECENT CLASSIFICATIONS OF THE GREEN ALGAE.

THE appearance of the fourth edition of Engler's 'Syllabus der Pflanzenfamilien' (whose preface is dated May, 1904, although so far as the green algae are concerned this edition does not differ from the third, dated July, 1902); Blackman and Tansley's 'Revision of the Classification of the Green Algae' (1903); West's 'Treatise on the British Freshwater Algae' (April, 1904), and Oltmanns's 'Morphologie und Biologie der Algen' (July, 1904) enables us to bring together in parallel columns the different systems of classification which they employ (see table). It will be seen that there is little agreement as to the taxonomic grade of the groups. There is even less agreement as to subdivision of groups, and least of all as to their arrangement.

In comparing these four systems it must not be forgotten that Engler's and Oltmanns's are general, including all algae, while that of Blackman and Tansley's includes the green algae only (excluding the Diatoms and Charales), and West's is confined to British freshwater algae (including the Diatoms, but not Charales). Engler sets off the Zygophyceae, Chlorophyceae and Charales as 'branches' (Abteilungen) coordinate with Archegoniates (Embryophyta Asiphonogama), and Spermatophytes (Embryophyta Siphonogama). These he subdivides into classes, and the latter directly into families. Thus the class Bacillariales contains the single family Bacillariaceae, including all the Diatoms. West divides Bacillariaceae (as a class) into two orders, and these into no less than fifteen families. Blackman and Tansley group the green algae into four classes upon a single character, namely, the cilia on the zoospores and gametes, resulting in four parallel lines (classes). Their 'series' are equivalent to 'orders' in other systems. In West's system the old group Chlorophyceae is nearly the same as Engler's, but with the addition of the Conjugatae. Oltmanns's system, as far as it can be made out from the first volume ('Spezieller Teil'), is much like West's and includes three larger groups (classes?), the second and third divided into lower groups (orders?) which in turn are divided into families. Oltmanns does not use the terms 'class' and 'order' in the volume at hand, and for this reason brackets are used in the table.

TABLE SHOWING OUTLINES OF CLASSIFICATIONS OF GREEN ALGAE.

I. (Engler).	II. (Blackman & Tansley).	III. (West) *	IV. (Oltmanns).
Branch ZYGOPHYCEAE. Class *Bacillariales.* Class *Conjugatae.* Branch CHLOROPHYCEAE. Class *Protococcales.* Class *Confervales.* Class *Siphonales.* Branch CHARALES. Class? *Characeae.*	Class *Isokontae.* Series Protococcales. Series Siphonales. Series Ulvales. Series Ulotrichales. Class *Stephanokontae.* Class *Akontae.* Series Desmidiales. Series Zygnemales. Class *Heterokontae.* Series Chloromonadales. Series Confervales. Series Vaucheriales.	Class *Bacillariacae.* Order Centricae. Order Pennatae. Class *Heterokontae.* Order Confervales. Class *Chlorophyceae.* Order Protococcoideae. Order Conjugatae. Order Siphoneae. Order Cladophorales. Order Microsporales. Order Schizogoniales. Order Ulvales. Order Chaetophorales. Order Oedogoniales.	(Class) *Heterocontae.* (Class) *Acontae.* (Order) Conjugatae. (Order) Bacillariaceae. (Class) *Chlorophyceae.* (Order) Volvocales. (Order) Protococcales. (Order) Ulotrichales. (Order) Siphonocladiales (Order) Siphonales. (Order?) Charales.

* The sequence is reversed here so as to facilitate comparison with the other systems. West begins with higher forms and proceeds from these to lower forms.

The class Stephanokontae of Blackman and Tansley includes the single family Oedogoni-

aceae. In their class Heterokontae the first 'series' purposely includes flagellate animals (*Chloramoeba, Vacuolaria, Chlorosaccus* and *Chlorobotrys*) 'since they represent the primitive organisms possessing Heterokontan characters, from which the next two series have been derived.' The series Confervales includes such organisms as *Chlorothecium, Mischococcus, Ophiocytium, Conferva* (of Lagerheim) and *Botrydium*. These authors include *Vaucheria* in a third series, thus widely separating this genus from other Siphonales (in the class Isokontae). This separation is not followed by either West or Oltmanns, who recognize the class Heterokontae as including the Confervales only.

On looking over the outlines of these four systems, that of Blackman and Tansley strikes one as quite the most radical. In order to be understood the position of the authors as stated in their introduction must be borne in mind, as follows: "The most fundamental of these modern conceptions is that which proposes to regard the Algae as consisting of a number of natural classes, phylogenetically independent of one another, more or less parallel in evolution, and each derived separately from the Flagellata. * * * These parallel classes are generally to be distinguished from one another by cytological characters, and more especially by differences in the organization of the zoospore, which is held to retain, throughout each class, most of the characteristics of its primitive flagellate ancestor. The most conspicuous of these differentiating characteristics of the zoospore are the nature of the assimilatory pigments, the character of the chromatophore, and the arrangement of the flagella."

If we exclude the Diatoms and Charales it is found that Engler recognizes 27 families of green algae; Blackman and Tansley, 44; West, 28, and Oltmanns, 37. Clearly, the algologists are no more agreed as to the limits of the families of the green algae than they are as to other points in the classification of these organisms.

THE CUP-FUNGI OF IOWA.

In a recent number of the bulletins from the Laboratories of Natural History of the State University of Iowa (No. 4, Vol. V.) F. J. Seaver publishes a valuable paper on the 'Discomycetes of Eastern Iowa.' In preparation for this work the author collected 'nearly one hundred species,' of which fifty are now described, the remaining being 'retained for further study,' in the hope that they may appear in a later paper. The species described are all old, the author having wisely refrained from adding new species. The books in which each species is described are cited in connection with each description, the lists resembling lists of synonyms, which they actually are in some cases. The descriptions and notes are good, and the plates (twenty-five in number) are excellent.

SEAWEED STUDIES.

Professor Doctor J. J. Wolfe contributes a cytological study of the red seaweed *Nemalion* to the October number of the *Annals of Botany*, accompanying his paper with seventy-five well-drawn figures. In addition to working out very clearly the structure of the complex chromatophore he finds reasons for concluding 'that *Nemalion* presents the essentials of an antithetic alternation of generations, and that the cystocarp is, therefore, the homologue of the sporophyte in higher plants.'

SARGENT'S MANUAL OF TREES.

This important book has just appeared from the press, and there has not yet been time for the preparation of a complete review, which must be deferred until a later issue. It need only be said now that in a neat volume of 826 pages the author has described and figured about 642 species and varieties, which occur in North America north of Mexico. For the first time the American botanist who is especially interested in trees has a portable manual which he can use in every part of the country.

Charles E. Bessey.

The University of Nebraska.

NATIONAL ACADEMY OF SCIENCES.

The annual stated session of the National Academy of Sciences was held in Washington April 18–20, 1905.

The following members were present during

the session: Messrs. Agassiz, Allen, Becker, Billings, Boas, Boss, Brewer, Brooks, Brush, Cattell, Chittenden, Councilman, Dall, Davis, Dutton, Emmons, Gill, Hague, Langley, Merriam, Mitchell, Morse, Newcomb, Nichols, Osborn, Peirce, Putnam, Remsen, Walcott, Webster, Welch, Wells, White, Wood and Woodward.

The following papers were presented:

EDWARD L. NICHOLS: 'The Mechanical Equivalent of Light.'

DR. H. C. WOOD and DR. DANIEL M. HOYT: 'The Effects of Alcohol upon the Circulation.'

ALEXANDER AGASSIZ: 'The Expedition of the U. S. Fish Commission Steamer *Albatross*, in charge of Alexander Agassiz, in the Eastern Pacific, Lieut. Commander L. M. Garrett, commanding.'

WILLIAM M. DAVIS: 'Resequent Valleys.'

WILLIAM M. DAVIS: 'The Geographical Cycle in an Arid Climate.'

W. W. CAMPBELL: 'A Catalogue of Spectroscopic Binary Stars.'

C. D. PERRINE (introduced by W. W. Campbell): 'Discovery of the Sixth and Seventh Satellites of Jupiter and their Preliminary Orbits.'

W. K. BROOKS: 'The Axis of Symmetry of the Ovarian Egg of the Oysters.'

John C. Branner, of Stanford University; William H. Holmes, of the Bureau of American Ethnology; William H. Howell, of Johns Hopkins University; Arthur A. Noyes, of the Massachusetts Institute of Technology, and Michael I. Pupin, of Columbia University, were elected members of the academy.

M. Henri Becquerel, of Paris, and Professor Paul von Groth, of Munich, were elected foreign associates.

SCIENTIFIC NOTES AND NEWS.

PROFESSOR E. B. FROST has been appointed director of the Yerkes Observatory by the trustees of the University of Chicago, in succession to Professor G. E. Hale, who gives his whole time to the establishment of the new Solar Observatory of the Carnegie Institution at Mt. Wilson, Cal.

DR. WILLIAM OSLER has been elected an honorary fellow of the Royal College of Physicians of Ireland.

THE Baltimore correspondent of the N. Y. *Evening Post* states that at the request of Miss Mary E. Garrett, the benefactress of the medical department of Johns Hopkins University, Dr. W. H. Welch, Dr. W. S. Halstead and Dr. H. A. Kelly will meet Dr. William Osler in London in June, to sit for a group portrait to be painted by John S. Sargent.

THE Vienna Laryngological Society appointed Señor Manuel Garcia, on the occasion of his one hundredth birthday, to be an honorary member of the society. Professor Chiari, the president, handed the diploma of honorary membership to Señor Garcia.

PROFESSOR JOHN F. JAMESON, head of the department of history at the University of Chicago, has been offered the post of director of the Bureau of Historical Research in the Carnegie Institution, Washington, D. C. This position is vacant through the return of Professor J. Lawrence Laughlin to the University of Michigan.

PROFESSOR BASHFORD DEAN, of Columbia University, plans to spend several months in Japan, where he will continue his studies on the development of the ancient sharks, Cestracion and Chlamydoselachus. He will be the guest of the Imperial University of Tokyo.

PROFESSOR H. S. GRAVES, director of the Yale School of Forestry, who has been in India, is expected to return next month.

DR. ALBERT F. WOODS, of the Bureau of Plant Industry, has been delegated to attend the Second International Botanical Congress, to be held at Vienna in June, and the International Congress of Agriculture at Rome.

DR. D. H. CAMPBELL, of Stanford University, will spend next year in an extensive trip through Europe, Africa and Asia. He expects to attend the International Botanical Congress at Vienna and the meeting of the British Association at Cape Town, and hopes to be able to make botanical investigations in the newly opened regions about the Victoria and Zambesi falls. He will then visit Bombay and Ceylon and will spend some time at the Botanical Gardens at Buitenzorg, Java, returning

to the United States by way of the Philippine Islands.

PROFESSOR WILLIS L. JEPSON, of the botanical department of the University of California, will spend next year in Europe and in the tropics, gathering material for the botanical museum at Berkeley.

DR. HARRY PERKINS, of the University of Vermont, has received an appointment for the summer at the Marine Biological Laboratory of the Carnegie Institution at Dry Tortugas, near Key West.

M. ED. CASPARI has been elected president of the French Astronomical Society.

PROFESSOR M. TRAUB has been appointed director of the newly established department of agriculture of Java.

Nature states that the Irish branch of the Geological Survey has been transferred from the Board of Education to the Department of Agriculture and Technical Instruction for Ireland. The work will be carried on under the immediate direction of Professor G. A. J. Cole.

MR. C. A. SELEY, mechanical engineer of the Chicago, Rock Island and Pacific Railroad, delivered an address before the students and professors of Purdue University on April 11. His subject was 'Framing of Passenger and Freight Cars.'

PRINCE PEDRO OF ORLEANS AND BRAGANZA, son of the Comte d'Eu, who has already visited that part of Central Asia, contemplates making a fresh tour in Chinese Turkestan.

AT a meeting of the Royal Geographical Society on April 10 Lieutenant-Colonel C. C. Manifold read a paper on the 'Problem of the Upper Yang-tsze Provinces and their Communications.'

ON March 28, Dr. Fridjof Nansen presented a paper before the Royal Geographical Society on 'Oscillations of Shore-lines'; discussion followed, which was taken part in by Sir Archibald Geikie, Sir John Murray, Admiral Sir W. J. L. Wharton, Mr. Peach, Mr. A. Strahan, Mr. Huddleston, Dr. Mill, Dr. Hull and others.

Nature states that portraits recently added to the National Portrait Gallery include those of Sir Charles Lyell, Charles Darwin and Professor W. Whewell.

Plans are being made to erect a memorial to the late president, Thomas M. Drown, of Lehigh University. It will take the form of a club house for the faculty and students, and will be erected at a cost of $80,000.

COLONEL NICHOLAS PIKE, known for his contributions to the natural history of birds, reptiles and amphibia, died at his home in Brooklyn, on April 11, in his eighty-eighth year. A naturalist of the old school, he was the author of interesting notices on the life-history and habits of a number of rare forms, especially among the amphibia. For several years he held the post of American consul in the island of Mauritius, and during this time he collected extensively the local fauna and prepared from the living specimens many colored drawings. Especially noteworthy were the albums of the fishes of the Indian Ocean, some of which illustrated species which were later described by Louis Agassiz. His most extended work was his 'Sub-Tropical Rambles in the Land of the Aphanopteryx.'

PROFESSOR LEBEN WARREN, for twenty-seven years professor of mathematics at Colby College, died on April 21, at the age of sixty-nine years.

MR. H. B. MEDLICOTT, F.R.S., director of the Geological Survey of India from 1876 to 1887, died on April 6, at seventy-six years of age.

WE learn from *The Experimental Station Record* that Professor Emerich Meissl, of the Austrian ministry of agriculture, died on February 15 at the age of fifty years. Professor Meissl was for more than twenty years connected with the agricultural experiment station at Vienna, being director from 1886 to 1898. At that time he was called to the ministry of agriculture as an agricultural-technological expert, and was promoted to the charge of a section in the ministry in 1902, which position he occupied at the time of his death. He was widely known among agricul-

tural chemists, having made many contributions upon agricultural analysis, and the chemistry of sugars, milk, and the fermentation industries.

DR. GEORG MEISSNER, formerly professor of physiology at Göttingen, died on March 30, at the age of seventy-four years.

AT a meeting of the council of the American Anthropological Association held in New York on April 15 it was voted to hold a special meeting of the association in Portland, Oregon, during the Centennial Exposition. The members of the council present were: Messrs. Boas, Chamberlain, Culin, Farrand, Gordon, Hodge, Hyde, MacCurdy, Pepper, Putnam, Saville and Smith.

The Experiment Station Record states that it has been decided to locate the new buildings for the Department of Agriculture 106 feet farther west, and to sink the structures 10 feet lower in the ground than was previously planned. This decision is in accordance with the plans of the park commission appointed by the senate some years ago. The details which have been worked out by this commission since the publication of their report make the above changes necessary in order to conform to the general scheme in the matter of the grade and the relative position of buildings. As the excavation for the two laboratory wings as originally located had been completed, these changes will involve some delay in the work.

THE King Institute of Preventive Medicine at Guindy was formally opened, on March 11, by Lord Ampthill, governor of Madras.

MR. ALFRED BEIT has increased his donation to the London Institute of Medical Sciences from £5,000 to £25,000.

WE learn from *The Athenæum* that at a recent meeting of the Institut de France the disposition of 30,000 francs forming the Debrousse legacy was the chief subject of discussion, and M. Poincaré's report recommended the following appropriations: Publication of the 'Tables de la Lune,' 5,000 fr.; *Journal des Savants*, 5,000 fr.; 'catalogue' of the works of Leibnitz, 3,000 fr.; for the study of the 'tuniciers' at Naples, 3,000 fr.; for the work in connection with the installation of the library at Chantilly, 7,000 fr.; and for the introduction of a seismographic apparatus at the Paris Observatory, 3,000 fr. The remaining sum of 4,000 fr. is carried over to next year's account.

THE New York Botanical Garden announces the following spring lectures, to be delivered in the Lecture Hall of the Museum Building of the Garden, Bronx Park, on Saturday afternoons, at 4:30 o'clock:

April 29.—'The Indian and his Uses for Plants,' by Mr. Frederick V. Coville.

May 6.—'The Pines and their Life History,' by Professor Francis E. Lloyd.

May 13.—'Botanical Aspects of Deserts of Arizona, California, Sonora and Baja California,' by Dr. D. T. MacDougal.

May 20.—'The Coralline Seaweeds,' by Dr. Marshall A. Howe.

May 27.—'Cuba,' by Dr. W. A. Murrill.

June 3.—'Vegetable Poisons and their Strange Uses,' by Dr. H. H. Rusby.

WE learn from *Nature* that at the annual meeting of the Iron and Steel Institute, to be held on May 11 and 12, the Bessemer gold medal for 1905 will be presented to Professor J. O. Arnold. The awards of the Andrew Carnegie gold medal and research scholarships will be announced; and the president, Mr. R. A. Hadfield, will deliver his inaugural address. The following is a list of papers that are expected to be submitted: 'Experiments on the Fusibility of Blast Furnace Slags,' Dr. O Boudouard; 'Recent Developments of the Bertrand-Thiel Process,' Mr. J. H. Darby and Mr. G. Hatton; 'The Application of Dry-air Blast to the Manufacture of Iron,' Mr. James Gayley; 'The Effect Produced by Liquid Air Temperatures on the Mechanical and other Properties of Iron,' Mr. R. A. Hadfield; 'The Cleaning of Blast Furnace Gas,' Mr. Axel Sahlin; 'The Failure of an Iron Plate Through Fatigue,' Mr. S. A. Houghton; 'The Continuous Steel-making Process in Fixed Open-hearth Furnaces,' Mr. S. Surzycki; 'Accidents Due to the Asphyxiation of Blast Furnace Workmen,' Mr. B. H. Thwaite; and 'The Behavior of the Sulphur in Coke in the Blast Furnace,' Professor F. Wüst and Mr. P. Wolff.

AN optical convention will be held in London from May 31 to June 3. In addition to papers that will be published in a volume, there will be an exhibition of optical and scientific instruments of British manufacture. Dr. R. T. Glazebrook, director of the National Physical Laboratory is president, and the vice-presidents include Lord Kelvin, Lord Rayleigh, The Earl of Ross, Sir Howard Grubb, Sir W. H. M. Christie and Sir Wm. de W. Abney.

REUTER'S AGENCY is informed that the Duc d'Orléans has organized a North Polar expedition which will leave for the Arctic under the duke's personal leadership next month. For the purpose of the expedition the *Belgica*, the vessel of the recent Belgian Antarctic expedition, has been secured, together with the services of Lieutenant Gorlache, who will again command the ship on the present occasion. The object of the expedition is not to reach the North Pole, and, according to present arrangements, the duke will not winter in the Arctic, although the *Belgica* will be provisioned for the event of her being closed in by the ice. The expedition will leave Norway probably on May 1 and proceed direct to Franz Josef Land, where it is believed that an attempt will be made to push northwards by way of a new channel. The duke's staff will include some French scientists and a number of Norwegian sailors. The duke will sail under the French flag. It is pointed out that as the season is early this year it is probable that the *Belgica* will find little difficulty in gaining the shores of Franz Josef Land. The scheme of pushing up a new channel is not unattended with danger, owing to the force with which the ice pack is driven down by the strong currents. It was owing to this cause that the *Eira*, of Leigh Smith's expedition, was sunk off Cape Flora and that the Duca degli Abruzzi's vessel was also pierced by the ice pack. No doubt a lookout will be kept for the members of the American Ziegler expedition, who are still in the Arctic.

THE sundry civil bill for 1906, passed by the last congress, contains an item of $200,000 appropriated to the United States Geological Survey for the purpose of gaging streams and determining water supply. With this sum it is proposed to continue the work of measuring streams in all parts of the United States and of collecting data that will be helpful in promoting water powers and irrigation projects, and valuable in determining the quality of water best suited for domestic and municipal purposes and for manufacturing enterprises. Estimates of the daily flow of important rivers are needed by engineers and investors, as is shown by the many requests for such information received from all parts of the country. It is believed that more than $5,000,000 is annually expended in new projects that are stimulated largely by facts that have been ascertained officially during years of careful observation.

THE New York *Evening Post* states that Dr. Otto Klotz, government astronomer of the Dominion of Canada, has been in Cambridge recently, arranging with the Harvard Observatory for a station to perfect his series of longitude observations made in the interest of the Dominion Government. This work was instituted upon the completion of the British transpacific cable a few years ago. Dr. Klotz and his party made longitude connections beginning at Ottawa, at Vancouver, Fanning Island, the Fiji Islands, Norfolk Island, Queensland, Australia and Sidney, N. S. W., where his series met a like series from Greenwich eastward to Sidney. This completed the circuit of the world for the first time in work of this character, an event that culminated actually on the night of September 27, 1903. The work involves the setting up of a firm pier of cement or brick at each of the stations, on the top of which is a point, the longitude of which is determined with the utmost possible accuracy. The observers' clocks at two stations are telegraphically connected during observation, and the error determined with extreme refinement. It is to set up such a pier at Harvard that Dr. Klotz has come, and he has been promised the hearty cooperation of Professor E. C. Pickering and his staff in carrying out his project. This step connects the Canadian transcontinental longitude se-

ries at one end with the American series, and ultimately there will be a similar connection established between Vancouver and Seattle, thus completing the loop.

Topographers and geologists of the United States Geological Survey will be at work during the coming summer in the region south and east of Tonopah, Nevada. A party of fifteen or twenty topographers under the direction of Mr. R. H. Chapman will go into the field about the middle of May. They will make surveys for three topographic maps. Two of these maps will be detail maps made by Mr. William Stranahan, one of the Goldfield district, which is 23½ miles southeast of Tonopah, and one of the Bullfrog district, which is about 60 miles east of Goldfield. The Goldfield map will cover approximately 40 square miles and will be drawn on a scale of 2,000 feet to the inch. Triangulation and leveling will be carried from Owens Valley to get control for the Bullfrog map, which will also be drawn on a scale of 2,000 feet to the inch. The third map will be a reconnaissance map of an area about 120 miles long by 90 miles wide, or about 10,000 square miles, south and southeast of Goldfield. It will include Goldfield in its northwest corner. The reconnaissance map will include part of the Death Valley. Levels for the control of all this work are now being carried forward from Mohave by a topographic party under the direction of Mr. R. H. Farmer. It is hoped that there will be an opportunity of running a level line to find the correct elevation of Death Valley. Mr. E. M. Douglas, chief of the western section of topography has computed that the lowest point in the Valley is 450 feet below sea level, which makes it the lowest point in the United States, but the elevation has never been accurately and incontestably determined. Geologic studies in these same Nevada areas will be prosecuted during the summer under the direction of Mr. J. E. Spurr. With the assistance of Mr. S. H. Ball, Mr. Spurr will investigate the general geology of the district covered by the reconnaissance map. Mr. Spurr will also make a special report on the geology of the mining camps in this area. A third report will have to do with the geology of the Goldfield district. Mr. Spurr will be assisted in this last inquiry by Mr. G. H. Garrey.

UNIVERSITY AND EDUCATIONAL NEWS.

A teaching observatory will be established by the Ontario government at the University of Toronto. Dr. C. A. Chant expects to visit the observatories of the United States to study their plans and methods.

The main building of Vanderbilt University was destroyed by fire on April 20.

The Suez Canal Company has voted 50 guineas to be announced at the banquet over which Mr. Chamberlain will preside on May 10, on behalf of the London School of Tropical Medicine, this being a gift in recognition of the school's services in the tropics.

The Geological Department of Colby College, Waterville, Maine, has been abolished by the trustees of the college, the reason assigned for the action being a financial one. Professor W. S. Bayley, who has been in charge of the department during the past sixteen years will therefore sever his connection with the institution at the close of the present college year.

Dr. Charles M. Bakewell, assistant professor of philosophy in the University of California, has been elected to a professorship of philosophy in Yale University.

Mr. Charles W. Brown, of Lehigh University, has been appointed instructor in geology in Brown University.

Fellowships in zoology and entomology at the Ohio State University have been granted respectively to Mr. C. F. Jackson, of De Pauw University, and Mr. W. B. Herms, of German Wallace College, Berea, Ohio.

Professor Walter König, of Greifswald, has accepted a professorship of physics in the University of Giessen.

The council of University College, London, has appointed Sir Thomas Barlow to the Holme Chair of Clinical Medicine, vacant through the resignation of Professor F. T. Roberts.